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OF
THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

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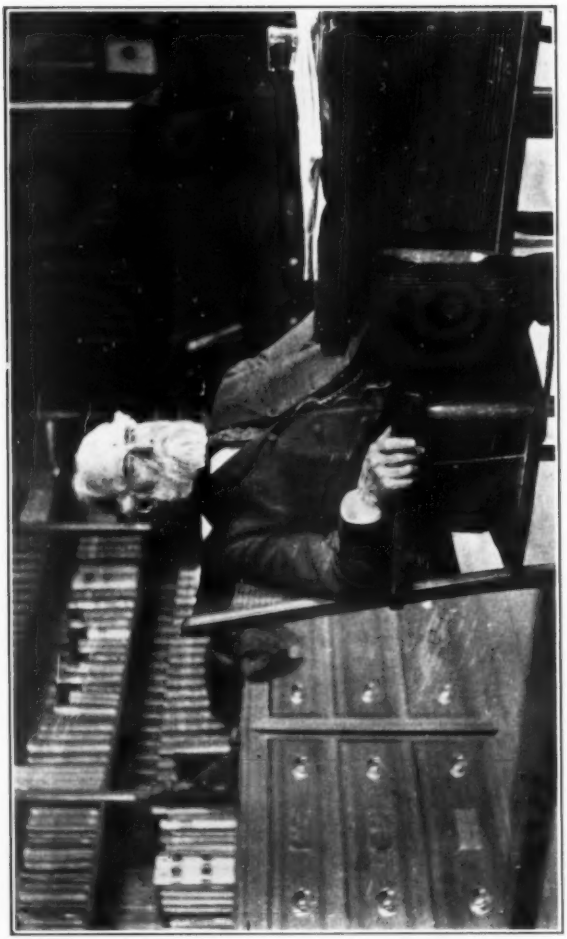

SIMON NEWCOMB

Born March 12, 1835; died July 11, 1909.

From a negative taken in the library of the Lick Observatory, August 7, 1907.

(*See Page 314*)

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VOL. III. JULY-AUGUST, 1909. No. 4

SOME REMARKS UPON THE DOCTRINE OF PROBABILITIES

BY W. F. KING

PROBABILITY in mathematics may be defined thus:

If an event may happen in a ways and fail in b ways, and if all these a and b ways are equally possible, then the probability of its happening is $\frac{a}{a+b}$, and the probability of its failing is $\frac{b}{a+b}$.

This in effect is Laplace's definition. The expression "equally possible," he explains to mean that we are "equally undecided" as to the several possible ways.

Thus if we throw an ordinary die, having six equal faces, we judge that all faces are equally likely to turn up, or we are equally undecided as to which will turn up. One only out of the six can turn up.

We therefore assess the probability of the appearance of a particular face, such as the six, for example, as $\frac{1}{6}$.

If a bag contains 12 balls, of which 8 are black and 4 are white, and one ball is drawn from the bag, the probability that this will be black is $\frac{8}{12}$ or $\frac{2}{3}$, and the probability that it will be white is $\frac{4}{12}$ or $\frac{1}{3}$.

So far it is a mere matter of definition. When we undertake to go a step further, and speculate upon the results of throwing the die a number of times, we encounter a difficulty in logic which has caused some logicians to deny the practical value of the doctrine built up by Laplace, and even to reject it altogether.

From our belief that the six events are all equally likely to occur, we infer that we may expect all to occur, in a long series of trials, equally often. This expectation of equal frequency is involved in our definition, for to assume that the ace, for example, should turn up more frequently than any other of the faces would be in contradiction of our hypothesis that all are equally likely to appear.

Hence, given equal probability, we expect equal frequency, and thereby we can calculate the probability of any combination of the different events.

Thus if we throw two dice, the probability of the two sixes is by the theory $^1/_6 \times {}^1/_6$ or $^1/_{36}$, and we therefore expect double sixes once in 36 throws, on the average of a large number of throws. If we toss five coins we find the probability of three heads and two tails to be $\frac{5 \cdot 4}{1 \cdot 2}\left(\frac{1}{2}\right)^5 = \frac{5}{16}$, whence we expect this combination to occur on the average five times out of sixteen trials. Similarly may the probabilities of other combinations be calculated.

Now, if we carry out the experiment, and find, as we probably shall, that the result is quite closely in accord with our expectations, we seem, from our professed want of knowledge as to which faces of the dice or which side of the coin will turn up, to have predicted, by mathematical deduction, an experimental fact; or, in other words, to have created knowledge out of our ignorance.

Jevons, in his *Principles of Science*, states that he tried the experiment of throwing ten coins 2048 times. On the assumption that the frequency of head and tail of each coin is equal, the mathematical theory gives the frequency of occurrence

of the different arrangements. This is given in the following table, with the actual result of Jevons's experiment.

Heads	Tails	Calculated	Observed	Difference	Difference Per Cent.
10	0	2	4	2	0·1
9	1	20	35	15	0·7
8	2	90	130	40	2·0
7	3	240	252	12	·6
6	4	420	371	−49	−2·4
5	5	504	489	−15	−0·7
4	6	420	398	−22	−1·1
3	7	240	230	−10	− ·5
2	8	90	102	12	·6
1	9	20	36	16	·8
0	10	2	1	− 1	−0·5
		2048	2048		

The total number of heads was 10,333; of tails 10,127. The agreement between theory and experience is fairly good.

Quetelet placed 20 white balls and 20 black balls in an urn, and drew out a ball at a time, replacing it before a new drawing. In 2,096 trials he got 2,066 white and 2,030 black balls.

In a series of throws of coin recorded by Buffon there were 2,048 heads to 1,992 tails; and in another, referred to by De Morgan, 2,044 tails and 2,048 heads.

In all these cases the agreement, though not perfect, is sufficiently close to make it appear that we have predicted an experimental result by abstract reasoning based on ignorance—our ignorance of which side of a coin will turn up.

The logicians of whom I have spoken object that the frequency with which a die or a coin turns up a particular face, like other properties of material things, cannot be determined by *a priori* reasoning. It must be ascertained by experience.

The only logical basis for determining the probability of any event is, they say, actual trial. We know that the coin turns either face indifferently, only because we know by experience that it turns head about as often as tails in a long series of trials.

It must be admitted that experience is the test of frequency, but we must distinguish between frequency and probability.

Frequency is the property of the things or events; probability is the state of our minds by which we expect the frequency.

The purpose of the calculation of probabilities is to measure the extent of our knowledge in order to estimate what our expectations ought to be, from the data before us. The final test whether we have judged correctly is experience.

We do not say that the coin will fall head and tail equally often *because* we do not know which will happen at any trial, but we assume that as a likely hypothesis, which subsequent experience seems to justify.

You will notice, however, that in all the experiments with coins which I have mentioned, there is a small preponderance of heads. This is a slight indication of a tendency in favor of heads, but this doubtless will be different with different coins. It would require an infinite number of trials to ascertain the probability by experiment. Thus the following definition, which has been proposed instead of Laplace's, is defective.

"Suppose we have a succession of things (or events or persons) each possessing some general characteristics, on account of which they can be classed together, and that amongst them is a section, each member of which, besides the general characteristics, possesses an additional attribute, which distinguishes this particular section from all the other things. Then, if, on taking any *large* number of the general things, we find that the number of particular things amongst them tends to bear to it a constant ratio, this ratio is called the probability of any one of the general being one of the particular things."

It may be proved that if the probability is, say $\frac{a}{a+b}$, as the number of trials is indefinitely increased the ratio of the number of favorable to the number of unfavorable cases will tend to approach $a : b$ as its limit.

The definition just given assumes the converse, but it would be a difficult task, I think, to prove by experiment that the ratio tended to a limit. It would at least require a very great number of trials.

If we toss a coin 200 times, theory indicates that we may be sure that head falls more than 70 times and less than 130, for the probability that the number lies outside these limits is only about 1 in 50,000. It is an even chance that it falls between 97 and 103 times.

If we increase the number of trials to 20,000, that is 100 times as many, the range of the possible number of heads is increased ten times from 60 to 600, that is, the number will lie between 9,700 and 10,300, while it becomes an even chance that the number of heads lies between 9,970 to 10,030.

Thus with increased number of trials it is not to be expected that the number of heads will approach closer to the theoretical value of one-half of the total number, though the ratio of frequency approaches ½ as its limit.

Thus it will be seen that a very great number of trials would be needed to determine the "probability" by experiment.

Of course, the coins, the dice, or the balls in the ballot box are used as illustrations merely. The theory is applicable to questions of greater importance, some examples of which I shall presently give.

The rules for calculating direct probabilities are:

If two events are exclusive, the probability of one or other happening is the sum of their respective possibilities. If two or more events are independent of one another, the chance of their occurring together is the product of their respective probabilities

The latter may be applied to the determination of the probability of the truth of the conclusion, deduced correctly from premises which are probable only.

Thus in syllogistic form:

If A is B
and B is C
then A is C.

If the probability of the first premise is p, and of the second q, the probability of the conclusion is pq.

Putting for example

$$p = \tfrac{3}{4},\ q = \tfrac{2}{3},\ pq = \tfrac{3}{4} \times \tfrac{2}{3} = \tfrac{1}{2}.$$

Thus the conclusion is less probable than either of the premises, a result according with natural judgment.

Hence a long chain of premises will give a conclusion of relatively small probability, since the probability is reduced at each step.

To arrive at the probability of a conclusion, which is based on several lines of argument, we proceed thus :

Let a, b, c, be the probabilities of the truth of the conclusion as derived by the process given above from the different arguments.

Then the conclusion is true unless the arguments *all* fail.

The chance that they all fail is

$$(1 - a)\ (1 - b)\ (1 - c),$$

and the chance that they do not *all* fail is

$$1 - (1 - a)\ (1 - b)\ (1 - c).$$

If we add an additional argument we shall have

$$1 - (1 - a)\ (1 - b)\ (1 - c)\ (1 - d).$$

This is greater than the former, since $1 - d < 1$, whence we see that every additional argument, however weak, adds strength to the conclusion.

A useful practical application of the direct calculus of probabilities is to the determination of the probability of errors of measurements.

The method of least squares, as used for the adjustment of observations, depends upon the assumption that the error of an observation is made up of a great number of small or elementary errors, each of which is as likely to be positive as negative. The problem is precisely analogous to that of Jevons, the throwing of a handful of coins.

The probability of errors of different magnitudes are, then, the coefficients of the expansion of

$$\left(\frac{1}{2} + \frac{1}{2}\ x\right)^n$$

or of

$$\frac{1}{2^n}\ (1 + x)^n,$$

the general term of which is

$$\frac{1}{2^n} \frac{\lfloor n}{\lfloor r \lfloor n - r} x^r.$$

When n is a large number, the calculation of the coefficients algebraically is simplified by substituting, for the factorials, their exponential equivalents, according to Stirling's theorem. This gives the usual transcendental form of the equation to the curve of probability.

We now proceed to the inverse problem of probability. In the direct problem which we have hitherto been considering, the probabilities of one or more events being known or assumed, we calculate the probability of any combination of these events. In the inverse problem we are given the result, which may depend upon one or other of several causes, (or antecedents), and it is required to determine the probability of the different causes.

If the *a priori* probabilities of the possible causes are P_1, P_2, P_3, ... respectively, and p_1, p_2, p_3 . . . . the probabilities that if the respective causes existed the effect would follow, then the probability that the first supposed cause was the cause of the observed effect is

$$\frac{P_1 p_1}{P_1 p_1 + P_2 p_2 + P_3 p_3 + \ldots}$$

This very easily follows from first principles. I shall not stop to prove the formula.

Thus if from a box containing 3 balls, which it is known are either white or black, one is drawn and found to be white, it is required to determine the probability that all are white.

We have here two hypotheses: (1) that the box contained 1 white and 2 black; (2) that it contained 2 white and 1 black; (3) that all the balls are white.

Each of these hypotheses is *a priori* equally likely; therefore the chance of each is $\frac{1}{3}$.

Under the first hypothesis the probability of the known effect, the drawing of a white ball is $\frac{1}{3}$; under the second $\frac{2}{3}$, and under the third $\frac{3}{3}$.

Hence the probability that the three balls are white is

$$\frac{1/3 \cdot 3/3}{1/3 \cdot 1/3 + 1/3 \cdot 2/3 + 1/3 \cdot 3/3} = 1/2.$$

Similarly the chance of 2 white and 1 black is

$$\frac{1/3 \cdot 2/3}{1/3 \cdot 1/3 + 1/3 \cdot 2/3 + 1/3 \cdot 3/3} = 1/3$$

and the chance of 1 white and 2 black is

$$\frac{1/3 \cdot 1/3}{1/3 \cdot 1/3 + 1/3 \cdot 2/3 + 1/3 \cdot 3/3} = 1/6.$$

The result in this case is seen to be in general accord with common notions. Having drawn 1 white ball from a box containing 3, we judge it to be more likely that the 3 balls are white than that 2 are white and 1 black, and this, again, more likely than that the 1 white ball drawn was the only one in the box.

The calculation gives the numerical valuation of this judgment.

Objection has been made to the calculation of inverse probabilities on the ground that the resulting probability is meaningless. Under the second definition of probability it would mean that if the effect occurred a very great number of times, it would be due to the different causes a number of times proportional to their respective probable errors. In most practical cases the experiment cannot be repeated, what then do we mean by assigning a probability to a past cause?

The answer to this must be as above in the case of the supposed ballot box; the probability is the measure of our judgment. In this case we judge the different hypotheses to be likely in the ratio of 1/2, 1/3 and 1/6 or as 3, 2 and 1.

Before going further it is necessary to show how we deal with effects of which the causes are unknown.

In such a case we assume that the probability may have any value from 0 to 1.

Suppose an event of which we do not know the causes has happened n times, what is the chance that it will happen once more?

Suppose p to be the unknown *a priori* probability of the event happening once. It has happened n times. The probability of this is p^n.

Assuming that all probabilities between 0 and 1 are equally likely, the *a posteriori* probability that the probability is p_1 is

$$\frac{p_1{}^n}{p_1{}^n + p_2{}^n + p_3{}^n + \ldots + p_r{}^n}$$

putting in the denominator all possible values of p and adding.

The chance that it happens once more is

$$\frac{p_1{}^{n+1}}{p_1{}^n + p_2{}^n + \ldots p_r{}^n}$$

if the probability is p_2, it is

$$\frac{p_2{}^{n+1}}{p_1{}^n + p_2{}^n + p_3{}^n + \ldots + p_r{}^n}$$

and so forth. Therefore the complete probability of its happening once more is

$$\frac{p_1{}^{n+1} + p_2{}^{n+1} + p_3{}^{n+1} + \ldots + p_r{}^{n+1}}{p_1{}^n + p_2{}^n + p_3{}^n + \ldots + p_r{}^n}$$

To give all possible values to p we assume that p varies continuously from 0 to 1, and sum by the integral calculus thus:

$$\frac{\int_0^1 p^{n+1} dx}{\int_0^1 p^n \, dx.}$$

This is equal to

$$\frac{n+1}{n+2}.$$

Numerous illustrations of this can be taken from the order of nature. The one usually given is the sunrise. Suppose a man, quite ignorant of the causes of the phenomenon, saw the Sun rise on 100 mornings, the chance to him that the Sun would rise next morning would be

$$\frac{100+1}{100+2} = \frac{101}{102}.$$

The chance that it would rise also on the morning after would be

$$\frac{n+1}{n+3} = \frac{101}{103},$$

a slightly smaller chance; and that it would rise 100 mornings more, just as long again as he had observed it, would be

$$\frac{n+1}{n+101} = \frac{101}{201},$$

very nearly ½. ½ is the expression of complete doubt, indicating that there are as many chances against the event as for it.

Hence we deduce that knowledge of nature by observation alone, without ascertaining the reasons why, is to be relied upon only when we do not depart far from the limits of our experience.

This formula

$$\frac{n+1}{n+2},$$

has been the central point of attack upon the inverse method. Thus Cajori, in his *History of Mathematics*, states that the best modern thinkers have rejected the doctrine of probabilities, and he refers to this formula apparently as a justification of his statement.

He says, granting this formula to truly represent the probability that the event which has happened n times will happen once more, it will follow that if n is put equal to zero, we shall find that the chance of an event happening, which we have never observed, and of which we know nothing, is ½. It follows, he says, that of all possible theories which can be propounded, one half must be true. This he seems to consider a *reductio ad absurdum*

As to the chance reducing to ½ when n is zero, there is nothing wrong in that. Probability equal to one-half is merely the expression of complete uncertainty. If we had never observed an event and knew nothing of its causes or of its previous occurrence, we could only be in doubt as to its happening. If asked, we could only say that so far as we knew it was as likely to happen as not.

As to one half of all possible theories being necessarily true, this seems evident. For every theory which may be propounded, there is another theory, the direct opposite, which may also be

propounded. The negative theories must be equal in number to the affirmative. One-half of the total number is true and one-half false.

I have lately seen this same *reductio ad absurdum* stated thus:

Suppose we have before us a number of similar boxes. We open n of them and find each one of them contains a mouse. The chance, then, by theory, that the next one opened contains a mouse is

$$\frac{n+1}{n+2}.$$

Hence if we have opened none of them, the chance that the first one we open contains a mouse is ½.

As the article in which I saw this argument did not profess to be humorous, I suppose the result was looked upon as a clincher.

Remember the theory presupposes complete ignorance. If we knew nothing of mice or their habits, I do not know whether, if asked whether there was a mouse in a particular box, we could say anything, but that so far as we can judge, it is just as likely as not, or that the chance is ½.

The calculus of inverse probabilities is frequently useful in deciding whether an observed occurrence is the result of coincidence merely, or is the effect of a definite cause.

Jevons thus discusses the question whether iron exists in the Sun. He refers to Kirchhoff and Bunsen's measurements of the spectrum, in which they identified 60 lines common to the two spectra. He estimates each coincidence of lines, with the imperfect observations, as having a probability of ½. That is if a line of the iron spectrum should as it were be thrown at random upon the solar spectrum, the chance that it would come near enough to a line on the latter to be counted as coincident, would be ½.

The probability that there should be sixty such coincidences by accident would be $(\frac{1}{2})^{60}$, an extremely small quantity, so small in comparison with the probability of the observed state of

things upon the hypothesis that iron exists in the Sun, as to make it practically certain that iron does exist there.

In mathematical language, if p be the *a priori* chance, that is the chance from other evidence that iron exists in the Sun, the probability when the new evidence is adduced is

$$\frac{p}{(\frac{1}{2})^{60} + p}.$$

Now unless p is so small as to be comparable in magnitude to $(\frac{1}{2})^{60}$ that is to 1 divided by a number of 19 places of figures, this probability is practically unity. That is, it is very nearly certain that iron exists in the Sun.

It should be noted that in thus deciding between accidental coincidence and a natural cause, it is necessary that we have some reason to think that the assigned natural cause is the only one which will explain the phenomenon.

Thus, in general, it is not sufficient proof of a theory that it explains a phenomenon. Many phenomena are capable of several different explanations. The theory adopted should have some probability from other evidence, and greater probability than alternative theories.

Suppose we undertake to explain a phenomenon by a cause of which we have no knowledge. To the existence of this cause we consequently assign the probability one-half.

The probability of other causes (also unknown) will be

$$\int_0^1 pdp$$

where p is given all values from 0 to 1, whence the *a posteriori* probability of our assumed cause is

$$\frac{\frac{1}{2}}{\frac{1}{2} + \int_0^1 pdp} = \frac{\frac{1}{2}}{\frac{1}{2} + \frac{1}{2}} = \frac{1}{2}$$

and we are still in complete doubt.

Other examples of deciding between accidental coincidence and a direct cause may be given.

The principle has been applied to the proof that different sets of logarithmic tables, printed at different places, have a common origin, by discovery of identical errors in them.

If in tables containing millions of figures, one or more identical errors are found, it is immensely more probable that the errors should arise from copying than that they should have happened by chance.

Yet there is an instance on record when three computers, working independently of one another, made, in a long calculation, exactly the same error. Thus even the most improbable things *may* happen.

In astronomy, it has been shown that the probability of two stars being so nearly in the same direction, as seen from the earth, as to appear as a double star, is very small on the assumption of proximity by chance. Struve estimated that the odds were 9570 to 1 against any two stars of the seventh magnitude falling within 4″ of each other by chance.

The odds against the conjunction of these stars by chance forming a triple star are 173,524 to 1.

The conclusion in such cases is strong that the stars are connected by a physical cause, such as gravitation, which has for many of them been proved.

Laplace, from the concurrence of direction of orbital movement and rotation of the Sun, planets and satellites, estimated that the odds were about 4,400,000,000,000 to 1 in favor of a common cause for this uniformity.

Laplace, again, found the probability that the sum of the inclinations of the planetary orbits to the ecliptic would not exceed by accident the actual amount for the planets then known to be about ·000,000,112,35, or about 1 in 9,000,000.

Combining this probability with the former one derived from the direction of motion, it becomes immensely probable that the solar system arose from some common cause.

It is stated that of all comets observed about one-half only have their direction of motion the same as that of the planets. This is what we should expect on the hypothesis of chance, and will so far support the opinion that comets have an origin outside the system.

In a magazine article by a well known astronomer, which I saw some years ago, reference is made to the great nebula in Orion, in the midst of which appears a remarkable sextuple star. Taking the area of sky covered by the nebula as equal to one square degree, which is about $\frac{1}{41,000}$th of the whole sphere, he gives this as the probability that the star falls apparently in the nebula by chance. The probability that the star is physically connected with the nebula is therefore high.

This conclusion is warranted, because we know from our observations of double stars and from our general astronomical knowledge that a physical connection between heavenly bodies near one another is possible.

Suppose we take a point of the sky which we find to be 78°, or any other angular distance, from the centre of the nebula, and within a square degree of that we find a remarkable star, the probability that the star should happen to be there by chance is exactly the same as that it should be upon the nebula, $\frac{1}{41,000}$. Yet we do not in this case presume a physical connection with the nebula.

The reason, of course, is that in this last case we assign to the *a priori* probability of physical connection a very small value, so much less than even the minute $\frac{1}{41,000}$, as to be practically zero.

Returning again to what I said a short time ago as to the necessity of considering all possible explanations of an observed phenomenon, I would refer to an article which appeared in the *Scientific American* about a year ago, written by a self styled "mathematician."

He was endeavoring to prove that the markings which are seen on the face of Mars are the work of intelligent beings. He speaks of the lines as forming a complete network on the surface of the planet, connecting together at spots. These spots he estimates at being 30 miles in diameter and 600 miles apart.

He now supposes cracks to start from each spot to the spots around it. The probability that the lines should strike the six surrounding spots by chance he calculates to be about 1/24,000,000,000.

Then he figures the chance that 200 spots should be thus connected. This he finds to be about 1 in 16×10^{259}, *i.e.* 1 in 16 followed by 259 naughts.

Of course, in making this calculation he has assumed a regularity of arrangement of the markings for which he has little warrant from the observations.

It is the amusing logic of his conclusion to which I especially desire to call attention. He says: "Reversely, this enormous number (16×10^{259}) to 1 is the chance that the lines are the outcome of a definite underlying plan not due to natural causes, unless we suppose natural forces of which we have no cognizance, and therefore no specific right to call in. The whole aim of science is to explain; it is only nescience that summons the unknown to its aid. But here we have an explanation at hand able and sufficient, to wit: local intelligence on the planet directing the positions of these lines. This takes the place in our present enquiry that the law of gravitation does in the movement of the planets, and in both the mathematical chances in its favor are so overwhelming as to constitute what we mean by saying a thing is proved."

What do you think of that? He thinks the only natural cause, apart from intelligence of the human order, which there can be for the phenomenon is a blind energy shooting cracks at random from one spot to another.

The inverse method may be used to estimate the probability of statements which are supported by assertions or other evidence.

The assertions of witnesses may be assigned a probability from an estimate of the ratio of the number of times they speak the truth to the total number of assertions they make. Thus if a witness speaks the truth 9 times out of 10, his veracity would be put at $^9/_{10}$.

If two witnesses concur in making a statement the probability that the fact is as stated is found thus.

There are two hypotheses.

(1) The fact is as stated and the witnesses both say that it is so. If p and q are their respective veracities, the probability of this hypothesis is pq.

(2) The statement is false, both witnesses lying. The probability of this is $(1 - p)\ (1 - q)$. Hence the probability that the first hypothesis is correct, that is, that the fact is as stated, is

$$\frac{pq}{pq + (1 - p)\ (1 - q)}.$$

If $p = q = {}^9/_{10}$ the value of this is

$$\frac{\left(\frac{9}{10}\right)^2}{\left(\frac{9}{10}\right)^2 + \left(\frac{1}{10}\right)^2} = \frac{81}{82}.$$

If the *a priori* probability of an asserted fact is a, and one witness (veracity p) asserts it, the new probability is

$$\frac{ap}{ap + (1 - a)\ (1 - p)}.$$

Similarly if there are any number of witnesses, the probability is

$$\frac{ap\ p_1 p_2 \ldots}{app_1 \ldots + (1 - a)\ (1 - p)\ (1 - p_1)\ (1 - p_2) \ldots}.$$

In the foregoing it is supposed that there is no choice for the witnesses except to assert or deny. The question is yes or no.

For example, suppose a ballot box to contain 1000 balls, of which one only is white, and all the rest black. After one ball has been drawn at random a witness, whose veracity is ${}^9/_{10}$, asserts that the ball which was drawn was white.

The deduced probability that it was white is

$$\frac{\frac{1}{1000}\,p}{\frac{1}{1000}\,p + \frac{999}{1000}\,(1 - p)} = \frac{p}{999 - 998\ p}.$$

If $p = {}^9/_{10}$, this probability is equal to $\frac{1}{112}$; so that the assertion is incredible.

Let us, however, suppose a case where there is a choice.

Suppose the 1000 balls to be numbered from 1 to 1000. One ball is drawn, and the witness says it was numbered 97. What is the probability that the statement is true?

The *a priori* probability is again $\frac{1}{1000}$.

Suppose the experiment to be repeated many times. In a large number of N trials, No. 97 is drawn $\frac{1}{1000}\ N$ times, and the witness says it was drawn $\frac{1}{1000}\ pN$ times.

In the same number of trials, No. 97 is *not* drawn $\frac{999}{1000}N$ times. Out of these the witness speaks falsely on $\frac{999}{1000}N(1-p)$ occasions. but since, when he gives the false number, he is equally likely to name any one of the 999 balls which have not been drawn, he falsely selects No. 97 on only $\frac{1}{999}$th of these occasions, that is, $\frac{1}{999} \times \frac{999}{1000}N(1-p)$ or $\frac{1}{1000}N(1-p)$ times. Hence the probability that the fact is as stated by the witness is

$$\frac{\frac{1}{1000}Np}{\frac{1}{1000}Np + \frac{1}{1000}N(1-p)} = p = \frac{9}{10},$$

that is, the probability is exactly equal to the credibility of the witness, and the large *a priori* chance against No. 97 being drawn plays no part in the result.

Here is a seeming paradox, which again has been a stumbling block. Suppose the white ball was numbered 97. Then if the witness states that the ball drawn was white, his statement is to receive very little credence; the odds are 111 to 1 against its truth. If, however, he says that the ball drawn was No. 97, though it is the same ball, the odds in favor of his truthfulness are 9 to 1.

There seems to me very little difficulty in resolving this paradox.

If the witness said the ball is white, we would at once say "that is very unlikely because it is only one out of a thousand." But when the witness says it is No. 97, what can we say against it? It is just as likely to be No. 97 as any other. We depend, then, solely on the credibility of the witness.

Thus the difference between the two results is accordant with common sense.

Objection has been taken against the application of probabilities to testimony, on the ground that it is impossible to assign a definite credibility to a witness's statement, for that will vary with circumstances, whether he has a personal interest in the matter, whether he may be mistaken, and so forth; the rules can therefore be of no practical use.

It must be remembered, however, that the same rules apply to any kind of evidence; they are not confined to credibility of human statements merely.

Another seeming paradox may be noticed.

Suppose n witnesses, the credibility of each of whom is p, agree in a statement. Then the probability of the truth of the statement is, by the formula:

$$\frac{p^n}{p^n + 1 - p)^n} = \frac{1}{1 + \left(\frac{1-p}{p}\right)^p}.$$

Now suppose $p < \frac{1}{2}$. Then $\frac{1-p}{p} > 1$ and as n increases the probability decreases.

Therefore the greater the number of such witnesses who independently make the assertion, the less likely it is to be true, and if an infinite number of them were to say it is so, it is perfectly certain that it is *not* so.

If this offends common sense it is because we are not accustomed to such liars. Every man speaks the truth oftener than he lies.

Another form of testimony is traditionary; A says that B has said that a certain event has happened.

If their respective credibilities are p and p_1, the statement may be accounted for by the event having happened and both A and B telling the truth, or by both lying. If the event has happened the chance that it is so reported by A is then $pp_1 + (1-p)(1-p_1)$. Again, the event may not have happened. B falsely states that it has happened and A reports him correctly, or B reports correctly and A quotes him falsely.

The probability on this hypothesis of the statement made is

$$p(1-p_1) + p_1(1-p).$$

The *a posteriori* probability of the truth of the statement is therefore

$$\frac{pp_1 + (1-p)(1-p_1)}{pp_1 + (1-p)(1-p_1) + p(1-p_1) + p_1(1-p)},$$

which is equal to $pp_1 + (1-p)(1-p_1)$ Compare this with the

probability of the truth of a statement which is made independently by both A and B, which is

$$\frac{pp_1}{pp_1 + (1-p)(1-p_1)}.$$

Suppose $p = p_1$. Then the probability of the truth of the traditionary testimony is

$$p^2 + (1-p)^2,$$

and of the concurring testimony

$$\frac{p^2}{p^2 + (1-p)^2}.$$

We naturally expect to find this last greater than the other, which it is if $p > \frac{1}{2}$. If $p = \frac{1}{2}$, the probabilities are equal. But if $p < \frac{1}{2}$, the probability of the truth of the traditionary is greater than of the concurring evidence.

If, then, the witnesses are in the habit of speaking the truth less than one-half the number of times they make a statement, the statement is more credible when one makes it on the authority of the other than when their evidence is given independently.

Returning from this digression to the two cases of direct testimony, we have found for the probability of an event, whose *a priori* probability was a and which was attended by a witness of credibility p, as

$$\frac{ap}{ap + (1-a)(1-p)} \text{ or } \frac{ap}{ap + (1-a)(1-p)\left(\frac{a}{1-a}\right)} = p,$$

according to the character of the event testified to.

The first formula would apply to a case where the testimony is to a fact intrinsically improbable. Thus if a witness asserts something which is contrary to a well established law of nature, we disbelieve him.

As John Stuart Mill remarks, "Whatever is contradictory to a complete induction is false." It is indeed hardly to be asserted that any of our inductions as regards the laws of nature are complete; the most we are able to say is that the probabilities in their favor are so great that a contradiction of them is unbelievable.

If any one were to tell us that he saw a heavy boulder fall upwards, without the intervention of any agent, we should disbelieve him, however much we might trust his veracity or his judgment in ordinary affairs.

Yet very high probabilities may be overcome by the concurrence of evidence. Suppose the odds against an occurrence, which is attested by several independent witnesses or several independent lines of evidence, are 999,999,999,999 to 1, *i.e.*, the probability of it is only $\left(\frac{1}{10}\right)^{12}$. Thirteen witnesses in its favor, having credibility $\frac{9}{10}$, will make the odds in its favor more than even, and one witness more would make the odds greatly in its favor. Of course, if we assign a higher probability than $\frac{9}{10}$ to the credibility of the statements of the witnesses, the same result would be obtained with fewer witnesses.

When the case is similar to that of the ballot box with the numbered balls, corroboration by additional witnesses increases the probability very rapidly.

Suppose when one ball had been drawn out of the thousand balls in the box, two independent witnesses concur in saying it is number 97. Suppose their credibilities to be p and p_1.

There are two hypotheses.

(1) That the ball drawn was No. 97, and the two witnesses concur in saying so.

This probability is $\frac{pp_1}{1000}$.

(2) That the ball was not 97 and that the two witnesses have both falsely asserted that it was.

The probability of this is $(1-p)(1-p_1)\frac{999}{1000}$ multiplied by the probability that in lying both witnesses select the same ball (No. 97) out of the 999 balls which were not drawn.

Thus the probability on this hypothesis is

$$(1-p)(1-p_1)\frac{999}{1000} \times \frac{1}{(999)^2} = \frac{(1-p)(1-p_1)}{999000}.$$

Hence the probability, after the statements have been made, that the ball was No. 97 is

$$\frac{\frac{pp_1}{1000}}{\frac{pp}{1000} + \frac{(1-p)(1-p_1)}{999000}} = \frac{999pp_1}{999pp_1 + (1-p)(1-p_1)}.$$

If we take $p = p_1 = \frac{9}{10}$, this probability is equal to

$$\frac{80919}{80920}$$

or more than 80,000 to one in favor.

Rather strangely, this probability does not very rapidly decrease with decrease of credibility of the witnesses, as did the probability in the case of the choice between white and black.

If the credibilities p and p_1 are each one-half, so that only one-half of each witness's statements are true, the probability that the ball, 97, named by them both is the ball that was drawn, is

$$\frac{999}{1000}.$$

It would require that the credibility of the two witnesses (supposed equal) should be only $\frac{1}{1000}$, (which is the same as the *a priori* chance that a particular ball is drawn) to reduce the probability after the fact to the value of the *a priori* probability $\frac{1}{1000}$.

The reason for this is that the probability that the witnesses when lying should choose the *same* lie out of 999 possible ones is very small.

From this we see that in cases of identification, if two witnesses independently pick the same man out of a large number, the presumption is very strong that they have chosen the right man.

An astronomical illustration may be given where this principle of identification is applied.

In the magazine article I have already referred to, the writer dealt with the question whether a central sun exists, about which the whole universe revolves.

He referred to a suggestion which had been made that Alcyone, one of the Pleiades, was the central sun.

He dealt with the question thus:—

Think of the immense distance at which lies the most distant star visible to us, probably many thousands of light years away. Conceive a sphere described about the earth as centre, having this radius.

Then he says as the Atlantic Ocean is to a single drop of its water so is the boundless extent of space to the immense sphere we have just conceived.

Now suppose that in the Atlantic Ocean there existed only one specimen of a certain microscopic species of plant. This may, he supposes, be anywhere in the ocean between the equator and the poles, and at any depth beneath the surface.

Then, he says, as well might a naturalist go down to the shore and dip up one single drop, and expect to find the single specimen in it, as might an astronomer think to find the central sun within a distance from our earth less than that of the star mentioned. In other words, the theory of probabilities, in his view, renders it infinitely unlikely that the central sun should be near enough to us to be within our cognizance.

Now I imagine that whoever made the suggestion that Alcyone was the central sun had some evidence to that effect or believed he had.

To work the probability out after the theory we proceed thus: Let p be the probability of the truth of the evidence that Alcyone is the central sun.

Let N be the number of stars of which, *a priori*, any one may with equal likelihood be the central sun. Or, otherwise, let all space be divided into volumes, each equal to the immense sphere above referred to, and let N be the number of such parts of space. If we make this second assumption, we will put p equal to the probability from evidence that the central sun is within the one sphere surrounding us. In either case the reasoning will be the same.

Let p, then, be the probability of the evidence in favor of Alcyone, and N the number of stars.

Assume $N = 10^x$, in order that we may have a convenient expression for a large number. Suppose $x = 1{,}000{,}000$. Then

N would be expressed in figures by unity followed by one million zeros, a number which, writing 500 figures every second for 8 hours a day, would take us over 4 days to write down. This will surely be a large enough number.

Then we have two hypotheses.

(1) That the star named is the central sun and that the evidence in its favor is true. The probability of this is $\frac{p}{10^x}$.

(2) That the star is not the central sun, and that the evidence in its favor is false. The probability of this is $(1-p)\left(1-\frac{1}{10^x}\right)$. But if the evidence is false, the probability that it should point to one particular star out of the 10^x-1 stars other than the central sun, is $\frac{1}{10^x-1}$.

Therefore the probability of the second hypothesis is

$$(1-p)\left\{1-\frac{1}{10^x}\right\}\frac{1}{10^x-1}=\frac{1-p}{10^x}.$$

Hence the probability that Alcyone is the central sun, is

$$\frac{\frac{p}{10^x}}{\frac{p}{10^x}+\frac{1-p}{10^x}}=p,$$

or the *a priori* probability from the evidence. The immensity of space, represented by the number 10^x, plays no part in the result.

It has already been pointed out that the fact that the probability of an event has a value equal to a certain fraction does not imply that in a finite number of trials, the event will happen exactly that fraction of the total number of trials. It is possible, under the theory, that the event should happen any number of times from one up to the total number.

All we can say is that the chance that it shall happen the number of times given by its probability is greater than the chance that it shall happen any other number of times, but it is not incredible that the actual number of times of happening should differ widely from the number given by the probability.

Nevertheless a continual tendency in one direction gives

some reason to think thar the *a priori* chance has not been correctly estimated.

Thus suppose a coin has been tossed 5 times and shows tail each time.

The probability on this evidence alone that the facility of tail exceeds that of the head is.

$$\frac{\int_{\frac{1}{2}}^{1} x^5 \, dx}{\int_{0}^{1} x^5 \, dx} = 1 - \frac{1}{2^6} = \frac{63}{64}.$$

There is, however, a strong *a priori* probability that the frequencies of head and tail are equal, and we could hardly say, on this evidence alone, that the coin was not equally balanced.

Should there be, however, any presumption for other reasons that the coin was not true, the probability of this may be combined with that of the above evidence according to the formula,

$$\frac{ap}{ap_1 + (1-a)(1-p_1)}.$$

Thus, in the experiments referred to in the beginning of this paper the number of tails were found to exceed that of the heads by about 2 per cent.

Hence we may put the frequency of tail on this evidence as equal to $\frac{101}{200}$. Thus if the coin has shown tail 5 times in succession, without once showing head, we have for the final probability that tail has the preference.

$$\frac{\frac{63}{64} \cdot \frac{101}{200}}{\frac{63}{64} \cdot \frac{101}{200} + \frac{1}{64} \cdot \frac{99}{200}} = \frac{6363}{6462},$$

which is a trifle greater than $\frac{63}{64}$.

Here a common fallacy may be referred to. Suppose a coin has turned head several times in succession, there is a tendency to think it is more likely to show tail next time. The probable basis of this idea is the feeling that the coin has to average its equal number of heads and tails, and therefore having had an

undue number of heads, it must come back quickly to its quota of tails.

Nevertheless, if the coin is true, the probability of head at next throw is precisely what it was at the beginning—one half.

Instead of its being more likely to show tail, indeed, there is a certain probability, as shown above, that it is unequally balanced and is therefore more likely to show head on the next throw.

This fallacy deceived so distinguished a mathematician as D'Alembert.

I shall conclude with a reference to a paper by Mr. J. Miller Barr, which appeared some time ago in the JOURNAL of our Society.

Certain stars are found by spectroscopic observation to have a varying velocity in the line of sight. This indicates that the body observed is in revolution about an invisible companion. Having a sufficient number of measurements of the radial velocity at different points of the orbit, it is possible therefrom to find the eccentricity of the orbit, assumed to be elliptical, and certain of the other elements.

Among these elements is the apse-longitude, the angle which the major axis of the orbit makes with the plane at right angles to the line of sight.

This angle is reckoned from 0° to 360° in such a way that it lies between 0° and 180° when that extremity of the major axis of the orbit which is the nearer to the centre of force lies farther away from us than the focus of the orbit, and between 180° and 360° in the opposite case.

Mr. Barr gives a list of thirty systems of which the data were at hand. In 26 of these the apse longitude lay between 0° and 180°, and in only 4 was it greater than 180°.

As there is apparently no physical reason why the direction in which these bodies lie, as seen from the earth, should have anything to do with the direction of the major axis of their orbits, the *a priori* probability that the apse should be on either side of the focus is one half, and we should expect that one half of the

apse longitudes should lie between 0° and 180°, and one half between 180° and 360°.

The probability that 26 should happen in one direction and 4 in the other is the coefficient of

$$x^{26} \text{ in } \frac{1}{2^{30}} (1 + x)^{30},$$

which is equal to about $\frac{1}{46000}$. Or, otherwise the probability, on the ground of these results, that the frequency of angles between 0° and 180° does not exceed one half, is about $\frac{1}{58000}$ or almost zero.

Hence arises a strong presumption that an important cause has been overlooked. Mr. Barr suggests systematic error of observations.

More recent data, however, including several binaries recently measured, considerably diminish the preponderance of apse longitudes less than 180°, and much weaken the presumption above referred to.

Nevertheless the existence of the preponderance, though it be not so great as was thought at first, should prompt enquiry. The investigation of residual effects, exhibited by application of the theory of probabilities is very fruitful to science.

THE OTTAWA SPECTROGRAPHS

By J. S. Plaskett

FROM the beginning of the astronomical work at the Dominion Observatory, the determination of the radial velocities of stars by means of the spectrograph has been, with the full approval of the Director, Dr. King, and in the belief that it was the most useful work possible with our telescope, the most important part of the programme.

The Observatory was occupied in the spring of 1905 but, owing to preparation for and absence on the Eclipse Expedition to Labrador and to other duties, it was not until the end of the year or the beginning of 1906, that any serious attempt at actual observing was made. The difficulties experienced in using the Brashear Universal Spectroscope for radial velocity work and the attempts made to remove them are fully described in this Journal.*

It was soon recognised that satisfactory work could not be done without an instrument specially adapted for this specific purpose, and a combined three-prism and one-prism spectrograph was designed in 1906. Its construction in the Observatory workshop was completed and it was brought into regular service in May, 1907. It has been continuously used from that date until the present, giving good satisfaction. However, it was soon found, as stated in the paper on Spectrograph Design in the last issue and more fully discussed below, that difficulties in changing from one form to another seriously limited the usefulness of the combined instrument. Accordingly a new type of single-prism spectrograph was designed by the writer and constructed in the Observatory workshop. It was completed and brought into use in March last, and has proved itself an accurate and efficient instrument.

* Vol. I., p. 104.

As a description of these instruments has not yet been published, it has been thought desirable to refer in the following pages to their special features, describing first of all the combined three and one-prism instrument still in use with three prisms, and then the new single-prism spectrograph.

In designing the first of these instruments I had no hesitation in availing myself of the most useful features in spectrographs then in operation, and in consequence it is, in its general form, modelled after the various Brashear spectrographs and the Bruce spectrograph of the Yerkes Observatory, with numerous original details and modifications suggested by experience with our first instrument.

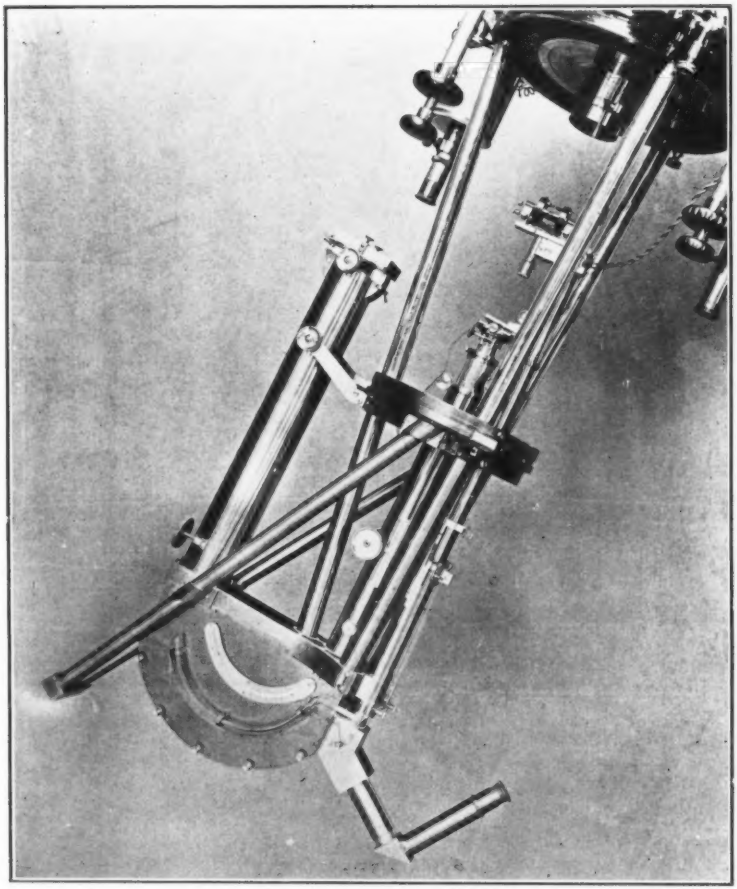
The Optical Parts

So far as the optical parts of the spectrograph are concerned it is not necessary here, after the full treatment in my previous paper on *The Design of Spectrographs** and in one recently published, *Camera Objectives for Spectrographs*,† to do more than mention the kind and dimensions of the various optical parts. The aperture of the collimator, which is a Hastings' triple Isokumatic, was chosen as 35 mm., which, with the aperture ratio of 1 to 15 of the equatorial, required a focal length of 525 mm. The prisms are of Jena Glass 0·102 which, with the ray at minimum deviation λ4415, chosen as the best compromise between the shortness of exposure around λ4500 and the better quality of spectrum for measurement around H_γ, require refracting angles of about 63° 45′ for a deviation of 60°. The lengths of the sides of the three prisms were made 75, 80 and 85 mm. respectively, and the height 40 mm. A separate prism of sides 75 and height 40 mm. was obtained for the single-prism form of the instrument. It was proposed in the first place to have cameras of 250, 375 and 525 mm. focus, but difficulties were encountered in obtaining the shorter focus objectives, and it was not until quite recently that one became available.† Consequently the instrument has been used entirely with a Zeiss

* This *Journal*, Vol. III., p. 190.

† † *Astrophysical Journal*, Vol. XXIX., p. 290.

PLATE XIII.



THREE-PRISM FORM OF THE OTTAWA SPECTROGRAPH

"Chromat" with three prisms, and a Brashear Single Material with one prism, both of these objectives being of about 525 mm. focal length.

The Frame of the Spectrograph

The principle followed as far as possible in designing the frame of the instrument was to so arrange the members that all stresses induced by change in position should act along them, and hence that any flexure could arise only by extension or compression of the parts and not by lateral bending. It consists (Plates XIII. and XIV.) essentially of two parts, the attaching truss and the spectrograph proper. The former consists of two ring-shaped castings united by three heavy-gauge steel tubes, the upper of these castings being attached to the end plate of the telescope by three swivelling clamps, while to the lower is clamped a similar casting forming the upper member of the spectrograph and permitting its rotation to any desired position angle. The frame of the spectrograph consists of the upper ring-shaped casting, mentioned above, and a heavily ribbed plate, which we may call the base casting, bearing the prisms and the camera (Plates XIII. and XIV.), united by the three inclined heavy brass tubes and a central tube in which the collimator tube slides. This truss is especially rigid owing to its large diameter at the upper end and to the practical union of the three outer and the central tubes at the lower end. The outer end of the base casting is rigidly supported by a diagonal tubular truss which, in addition, serves the double purpose of ensuring rigidity to the objective end of the camera with three prisms (Plate XIII.), and to the outer end of the camera with a single prism (Plate XIV.) The upper end of the camera, when the instrument is used with three prisms, is rigidly connected to the upper ring by the ribbed tie shown (Plate XIII.)

As will be readily seen, deformation due to flexure can only occur practically through the extension and compression of the tubular members of the truss, and will be the minimum possible for this form of the instrument. Although the tubes of the upper truss are of steel, the whole of the spectrograph proper is

of bronze and brass. One material is necessary to prevent differential expansion with temperature changes, and these were chosen on account of greater ease of working. The tubes are of hard-drawn brass and the castings of aluminium bronze, which is considerably stiffer than ordinary bronze or gun metal.

Attachment of Prisms

As Plate XV. shows, both the prism attachments, and cells are made very substantial in order to prevent any relative motion of the prism with respect to the rest of the instrument. The braced form for both one and three prisms, with the amount of metal present, will effectually prevent any flexure, and will also add to the stiffness of the base casting to which they are firmly screwed. The prism cells are made in one piece, and at the refracting edge are united by a rod of brass. The prisms are fastened in the cells by the gentle pressure of three adjusting screws on a plate, blotting paper being inserted between glass and metal on both top and bottom. The exact adjusted position of the prisms is maintained, without undue pressure, by narrow brass strips screwed to the base of the cell, and consequently no effect on the definition need be feared.

The Collimator

The collimator objective, Hastings' "Isokumatic," is mounted in a tube 44·4 mm. (1¾ inches) diameter and about 48 cm. (19 inches) long. This tube is movable by rack and pinion over a range of 80 mm., read by index and scale, in the central 51 mm. (2 inches) diameter tube. This movement is to enable the slit to be accurately set to the focus of the main objective. When set it is clamped firmly at both ends. The slit which was very satisfactorily made for us by Brashear has jaws of speculum metal inclined at an angle of 3½°. The slit tube which slides in the upper end of the collimator tube is graduated in millimetres, and is provided with tangent motion to enable the lines to be accurately set perpendicular in the direction of the spectrum.

The Cameras

A Zeiss "Chromat" objective, 45 mm. aperture, 525 mm. focus, is used with the three prisms, and a Brashear Single

Material of the same aperture and focus with the single prism.* To enable the instrument to be readily changed from one form to the other, separate cameras were made for each, and the single-prism camera remained permanently attached to the prism casting (Plate XV.) Neither of these objectives is corrected for color, and consequently the plate has to be inclined about 16° from the normal. The special construction of the camera back thereby required is shown in Plates XIII., XIV., and XV., and consists of a semi-cylinder to which the camera back is attached, rotating in an outer semi-cylinder, and provided with graduations and means of firmly clamping. The camera back has, in addition, a transverse motion in ways of some 15 mm., and can be rigidly clamped in any desired position. This motion is very convenient as it enables a number of spectra to be made side by side, on the same plate, for comparison in focussing or for other purposes. The camera tubes are of brass, 76 mm. (3 inches) in diameter, and the camera objectives are attached to inner tubes, movable by rack and pinion for focussing, with clamp screws for firmly fastening to any position. This position is read on a scale and vernier to tenths of a millimetre. The upper ends of the tubes are firmly connected to the ring casting in the one case (Plate XIII.), and to the ends of the diagonal truss in the other (Plate XIV.), by substantial webbed castings to prevent both longitudinal and lateral displacements.

The Plate Holders

The design of the plate holders differs from the usual type in which the plates are supported at the ends only, as tests had shown that successive plates did not lie in the same plane owing to curvature of the glass surfaces. An invariable position of the sensitive surface with respect to the camera back is necessary in order to secure the accurate focus so essential in radial velocity work. Consequently the method of supporting the plate in the holders was entirely changed, and it now rests, as seen in Plate XV., on a raised portion on the sides of the opening—13 mm. wide, 76 mm. long—in the front of the holder, which is closed

* For a description of these objectives and the form of their field, see *Astrophysical Journal*, Vol. XXIX., p. 290.

by a slide. This raised portion consists of two strips about 50 mm. long at each side of the opening. As the spectrum is not more than 6 mm. from the supports, it is evident that any curvature of the glass will have little effect on the position of the sensitive surface. To enable the plate holder to be loaded, it opens in the middle (Plate XV.), similarly to the English book form. The plate is placed face down, and the closing of the holder brings the spring, seen on the back, into action, firmly pressing the plate against the supports. Great care was taken in their construction to ensure that the distance of the sensitive surface from the front of the holder was exactly the same in each.

The Guiding Mechanism

The Brashear Universal Spectroscope used the star light transmitted through the slit, and reflected from the first prism surface, for guiding. This was not found altogether satisfactory, and in designing the new spectrograph it was thought desirable to follow the plan used by Frost in the Bruce Spectrograph, and combine this method with the Huggins method of using light reflected from the inclined slit jaws. In order to use the same guiding telescope in both cases, it was necessary to bring the star light reflected from the slit down to the same position as that coming from the first prism surface, and this is effected by three reflections. Two small right-angled prisms in the box, seen just below the comparison apparatus (Plate XIII.) reflect the light down through a tube parallel to the collimator tube. An objective placed near the upper end of this tube at its focal distance, along the optical part from the slit, renders the light parallel. An adjustable plane mirror in the angle box at the lower end of the tube, to which the bent guiding telescope is attached, reflects the light into the latter in the same direction that light reflected from the first prism surface enters when the mirror is turned up out of the way. Consequently to change from one form to the other it is only necessary to move the adjustable mirror above mentioned. In practice it has been found that the Huggins method is much superior, and it is always used in our regular observing.

The Comparison Apparatus

Owing to the change in star focus with changes in temperature, and to the corresponding change in slit position, and especially to provide for larger changes due to change of correcting lens, the upper parts of the guiding attachment and the slit diaphragms must be movable vertically, and this is effected very conveniently by attaching them to a tube, movable by rack and pinion in the upper part of the guiding tube. The comparison apparatus, well shown in the plates, is mounted on a brass plate, swivelling between centres in the end of the box carrying the reflecting prisms, and when not in use is turned back out of the way of the star light. Four sets of electrodes are mounted in a hard rubber wheel, any one of which can be readily used. We have, however, chosen an alloy of iron and vanadium as the most generally suitable, and it is invariably used, as all the tables required in the reduction of the measurements are computed with comparison lines of this alloy. A short tube, bearing at the lower end a condensing lens and at the upper end a piece of ground glass, is screwed into the plate directly under the electrodes and in the axis of collimation, serving to condense the comparison light on the slit. The angular aperture of this lens is more than double that of the collimator, and this, in conjunction with the diffusing screen and the permanence of adjustment of the apparatus, ensures uniform illumination of the collimator by the spark light. The spark is produced by a step-up transformer, 100 to 10,000 volts; the spark being thickened by a plate condenser containing 36 plates about 25 × 30 cm. in size, arranged so that 12, 24, or 36 plates may be used. Self-induction, for removing air lines and continuous spectrum, consisting of 100 turns of heavily insulated No. 12 wire, wound in three layers over a hollow cylinder in which iron may be placed, is included in the circuit. The resulting comparison lines leave nothing to be desired for sharpness, and as the primary wires are led to a switch at the eye end of the telescope, the comparison may be applied without leaving the eyepiece.

The Slit Diaphragms

Experience showed that adjustable occulting diaphragms, or windows for making star and comparison spectra of the proper width and in the proper position, were more or less of a nuisance, and, in the present case, diaphragms of fixed size, making the star spectrum 0·25 mm. wide and the comparison spectra 1·0 mm. wide, separated from the star spectrum on each side by spaces about 0·10 mm., are wholly used. These diaphragms are mounted on opposite sides of an opening in a plate which is moved transversely in ways across the slit by a knurled wheel. A quarter of a revolution of this wheel—the motion of the plate being limited by stops—changes from star to spark diaphragm. In consequence the operation of applying comparison is exceedingly simple. Turn down the comparison apparatus, turn this knurled wheel, throw on the switch, then reverse operations. As with single prism the total comparison exposure required is from 5 to 10 seconds, the whole operation from exposing star spectrum to again exposing star spectrum need not take more than 10 seconds. The whole slit diaphragm mechanism is attached to the guiding tube, and as it does not touch the slit head there is no possibility of any line displacement being caused by the movement of the diaphragm.

The Temperature Case

The importance of close temperature regulation in radial velocity work can not be over estimated. A change of temperature of 1° C. in the optical parts causes a displacement of the spectral lines equivalent to a velocity of about 20 km. per second, and this may be added to by expansion of the metal parts. Consequently from the beginning of accurate radial velocity investigations, considerable attention has been bestowed upon protecting the spectrograph from temperature changes. At first the instrument was merely wrapped with non-conducting materials, as felt or blanket, but such a method only rendered the changes slower. The next step was to surround the instrument with an outside case, in the interior of which coils of wire

enabled any fall of temperature to be overcome by passing a current through them. Finally, an automatic means of regulating this current was applied. In this spectrograph the tubular parts of the frame are covered with felt to smooth down any irregularities in the temperature regulation, and the whole spectrograph proper, except of course the slit head and guiding apparatus, is enclosed in a case of sheet aluminium, lined with felt, inside which is stretched single-silk-covered German silver wire. At first, following Hartmann's method, the wiring was confined to the sides of the case beside the prism box, but it was found preferable by experience to distribute it more uniformly over the whole interior of the case. There is altogether about 1200 ft. of No. 28 wire in the case, with a resistance of about 1200 ohms., divided into four circuits of 300 ohms. each. These four circuits are coupled in multiple two and two, giving two circuits, one for each side of the case, having in each about 600 ft. of wire with a resistance of 150 ohms.

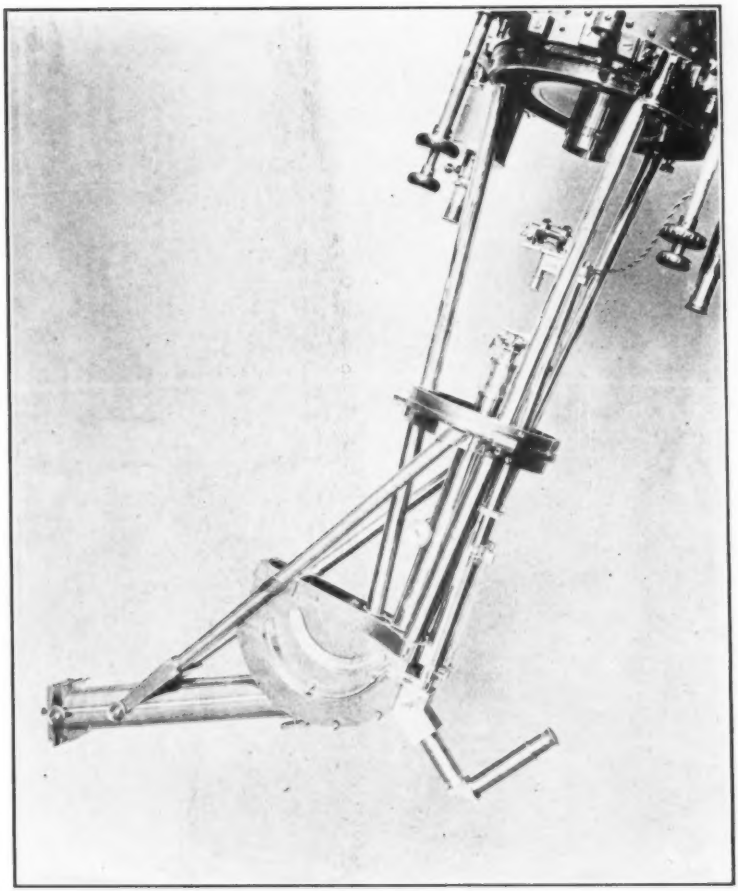
For automatically turning the current on and off these coils, a pair of electric contact thermometers, with wide bore capillaries open at the top and long semi-circular shaped bulbs, are attached one to each side of the prism box (Plates XIII. and XIV). One platinum wire is sealed into the upper part of the bulb, and a second one, adjustable up and down by rack and pinion, dips into the mercury in the capillary in each thermometer. A high resistance coil is connected across the gap to prevent undue sparking, and the coil of a 300-ohm relay, with two cells of dry battery, is connected in series with each pair of contacts. The regular lighting circuit is connected through the points of these relays with the two heating circuits in the case, an adjustable resistance being inserted to vary the amount of heat required to suit the difference between internal and external temperatures. In each of the heating circuits is placed a 5-volt miniature lamp which, by its glowing, shows when the heating current is applied, and the alternate lighting and extinguishing indicating the proper working of the apparatus. The relays, batteries, resistance, controlling switches and lamps are placed in a box, which is

permanently attached to the south side of the telescope pier, directly above the transformer, condenser and self-induction. The wires from and to the thermometers and heating coils, with the primary and secondary wires for the comparison spark, are all bound together into a flexible cable, running up the pier across to the tube and down it to the eye-end, and are provided with plugs for easy attachment and detachment. There are, in consequence, no wires whatever across the floor, avoiding all fear of tripping, entanglement or short circuiting. Owing to the large scale of the thermometers, they readily respond to changes of temperature of 0·1° C., and the temperature inside the case is consequently maintained uniform within that range. The working of the control is very satisfactory, causing no trouble whatever, and it is so sensitive that the heat is turned on and off two or three times a minute under normal conditions. It is found, however, that although the temperature inside the case undoubtedly remains constant, the temperature in the interior of the prism box gradually falls with continuously falling external temperature at the rate of about 0·1° C. per hour. This is probably due to conduction through the external metal parts of the instrument, and could probably only be overcome by also heating them or by suitably raising the thermometer wires, and consequently the temperature of the case, as the external temperature, diminishes.

The Spectrograph in Practice

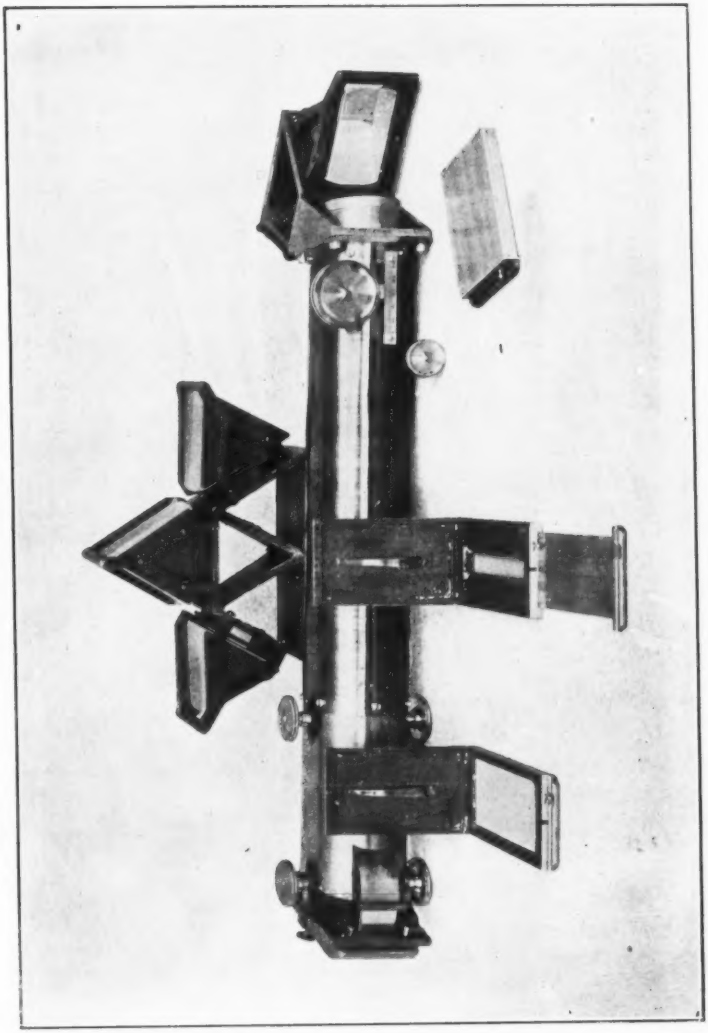
The spectrograph has now been in regular use for over two years, has been found very convenient in operation, and has given accurate results. It has been found by experience, however, that a combination type of instrument like the present, which can be used with one or three prisms, is neither as convenient nor desirable as two entirely separate instruments would be for our programme, which entails the use of both high and low dispersion. Although the time occupied for changing is not such a serious matter, from 15 to 20 minutes, the question of temperature introduces a difficulty. In making the change the outer case is of course removed, and, owing to the lower tem-

PLATE XIV.



ONE-PRISM FORM OF THE OTTAWA SPECTROGRAPH

PLATE XV.



PRISMS, CAMERA AND PLATE HOLDERS

perature of the dome, considerable uncertainty as to the temperature conditions in the changed form exists. In consequence, unless the instrument stands for an hour or so, some doubt is felt in regard to the reliability of the first spectrum obtained. Moreover, owing to the extended form of the single-prism instrument, some difficulties with uniform temperatures in the case and with flexure were experienced.

Under these circumstances, and with the ready assent of Dr. King, the designing and construction of a separate single-prism spectrograph was undertaken, with the intention of using the present instrument wholly with three prisms with both long and short focus cameras. The difficulties mentioned above with the single-prism form do not apply to the three-prism instrument, and its good qualities for this purpose are well established.

The New Single-Prism Spectrograph

The principles upon which the design of the new spectrograph, both optical and mechanical, is based were fully entered into in the last issue of this Journal, and in this place, therefore, only a short description of the instrument, in so far as it differs from the one just described, is necessary.

The Optical Parts

The Collimator Objective is a Hastings-Brashear "Isokumatic" of 51 mm. aperture and 765 mm. focus. The middle component of the "Isokumatic" is unfortunately yellowish in color, but it was felt to be preferable to use it rather than the ordinary triplet which, though less absorbing, has not so flat a color curve.

The prism is of Jena Glass 0·102, of practically identical optical constants with the glass used in the other prisms. The length of the side was made 110 mm., to entirely receive the 51 mm. diameter pencil, and the height 57 mm., in order that any effect produced by the pressure or unequal temperature of the supports might be minimized. The refracting angle 63° 30′ was computed for a deviation of the central ray, λ4325, of 60°. The ray at minimum deviation was taken more towards the

violet in this case, on account of the poor values obtained in the measurement of $H\beta$ in a recent investigation on the "Effect of Slit Width, etc."*

These measures showed such irregular velocity values for this line as to lead to the conclusion that it would be inadvisable to use it. If $H\beta$ is omitted from early type stars, for which this instrument is wholly intended, the range used for measurement will in general extend from K, λ 3934, to about λ 4600 in which the central ray chosen, λ 4325, is nearly midway. Further by raising the correcting lens slightly the minimum focus is shifted towards the violet and nearly coincides with λ 4325 and at the same time the foci for K and λ 4600 are equidistant, each 2·5 mm. beyond it. The most uniformly exposed spectrum along the whole range used can be obtained when the slit is set about 1·7 mm. below the minimum focus. This position is also the one in which possible displacements of the lines, due to the slit not being at the star focus for all wave lengths will be most likely to neutralize one another. The central ray chosen is thus probably the best one possible for the range under investigation.

The camera objective is a Brashear Single Material of 58·7 mm. aperture and 455 mm. focus. A description of this objective with the form of its field is given in a recent paper† and it need not be further discussed here.

The Mechanical Parts

As outlined in the previous paper the instrument consists essentially of two parts :

1. A rigid hollow triangular-shaped steel box containing at the obtuse angle the prism, and at the acute angles the slit and plate and comprising the spectrograph proper.

2. The T-iron frame or cradle attached to the end plate of the telescope in which the spectrograph proper is flexibly supported and which serves to keep it collimated without the possibility of flexure in the support inducing any stresses in the box itself.

* *Astrophysical Journal*, Vol. XXVIII., p. 259.

† *Astrophysical Journal*, Vol. XXIX., p. 290.

The Spectrograph Box

The box consists of two triangular-shaped plates of hard saw steel 1·7 mm. thick, forming what may be called the top and bottom, while the sides consist of plates of the same material and thickness 79·4 mm. (3⅛ inches) wide. In addition there are a number of internal braces and supports well shown in Plate XVI., which gives a good idea of the construction of the box. These braces as well as the sides have pieces of small angle-iron securely riveted along both edges to which the top and bottom are firmly screwed. These angle-irons are not shown in the figure as, to prevent internal stresses, the frame was put together, the angles then rivetted along all edges and the top and bottom plates screwed to the angles and to the internal castings, the heads of the screws on one side being seen in Plate XVII. The iron castings *A*, *B*, *C*, *D*, *E*, *F*, Plate XVI. are planed to exactly the same width as the sides and braces. *A* may be called the main casting as through its central hole the main support of the spectrograph passes. In the two lugs projecting from the triangular part the collimator and camera tubes slide, while *D* and *F* form the other bearings of these tubes, *C* and *E* being bored of larger diameter so that the collimator tube does not touch them. The upper support of the spectropraph is attached to the centre of *C*, and the third support to *B*, which also forms the connection between the box proper and the camera end, which has been made separate to permit of the use of camera objectives of different focal length.

The prism is mounted in a separate cast-iron cell but is insulated from the metal by hard rubber facings 3 mm. thick, It is kept in position by hard rubber stops and held firmly, yet without undue pressure in its seat by three small clamp screws passing through the top of the cell and bearing upon the upper hard rubber plate. The base of the cell is surfaced flat and is held securely in its compartment by five screws passing through slotted holes in the bottom plate, thus permitting adjustment for minimum deviation.

Collimator and camera tubes are provided with racks and pinions and clamping screws. Their position is read on millimetre scales, the latter having a vernier reading to tenths of a millimetre. These screws and scales can be seen in Plate XVII.

The camera attachment, whose general form and construction can be obtained from Plate XVII. has top, bottom and sides made of the same material as the box and firmly screwed to the casting *B*, Plate XVI. forming what is to all intents and purposes, one continuous box. The camera back swivels and moves transversely similarly to the one previously described and uses the same plate holders.

As will be readily seen from its form, construction and material, the spectrogragh box is exceedingly rigid and flexure produced by changes of position, however it was supported, would be very small. This flexure, however, is reduced to a vanishingly small quantity by the new supporting system used in this instrument in which the box is, as it were, floated in its cradle.

The Supporting Cradle

This truss, which is made of 1⅜ inch *T*-steel is attached at the upper end to a heavy ring casting fastened to the end plate of the telescope by the same swivel bolts used with the other spectrograph. The form of the truss is well shown in Plate XVII. and no description is necessary. It is evident that the flexure of this truss in the plane of the box will be negligibly small. Owing to the necessity of separating the pairs of trusses by about 20 cm. to allow the spectrograph with outside temperature case between them, the flexure in the transverse direction will be somewhat greater. This is minimized by uniting the two ends by a solid webbed casting and introducing cross braces at the upper end of the truss. However, at the small hour angles generally used, the component of the weight in this direction will be very small and the flexure negligible. As will be seen presently no flexure of the cradle can introduce any stress in the box and the only effect will be to slightly alter the axis of collimation of the spectrograph, which, as it can only take place

practically parallel to the refracting edge of the prism, and that only to a very small amount, can not affect the positions of the spectral lines.

The box is supported in this truss by three shafts. The main and central shaft, which is 1 inch (25.4 mm.) diameter, has a bearing on the box only about 1 mm. long in the centre, which with other points at the ends of the hole permits swivelling of 3 or 4 degrees but no rotation around the axis of collimation. The upper supporting shaft is also attached only at the centre of the box and allows longitudinal motion as well as swivelling in every direction, while the third supports, attached rigidly to each side of the box, are flexibly attached to the truss by counterbalancing weights, the levers being universally pivoted as well as slotted to allow free motion in every direction. The box is hence carried flexibly and equably in the cradle, the counterbalances being computed to carry their proper proportion of the whole weight without any possibility of the bending of the latter introducing any stress in the former and flexure, as will be later seen, is entirely obviated.

Accessories

Guiding in this instrument is provided for only by the Huggins method, the light from the slit jaws being reflected horizontally through a collimating objective into the bent guiding telescope. This gives a much more direct path and probably better definition than in the other instrument although in some positions of the telescope the guiding eyepiece is not so conveniently placed.

The comparison apparatus is similarly arranged to that already described except that in this case only one set of electrodes is provided for.

The slit with $3\frac{1}{2}°$ inclined reflecting jaws was made by Brashear, has one jaw fixed, the other movable micrometrically, and is provided with tangent screw for orienting. The slit diaphragm mechanism is precisely similar to that already described, but is attached in this case to the solid end of the collimator

tube. As this is firmly clamped in its bearing no chance of displacement is possible with the slight force required to move the diaphragms.

The Temperature Regulation

Besides avoidance of flexure, another reason for making the new instrument in box form was the better chance of efficient temperature regulation and the smaller chance of any slight irregularities producing differential expansions of the metal frame. The whole spectrograph box is in this case covered with a layer of felt ½ inch thick, sewed on; felt doors being provided for focussing scales, etc. The temperature case is constructed of thin pine lined entirely with felt, all joints being packed by the same material. The air space between box and spectrograph, or rather between felt lining and covering, is of uniform thickness, about 2·5 cm. throughout and the heating wires are uniformly distributed over the interior. The electric contact thermometers, not attached when the photograph was made, are placed, one on the right hand side near the top, and the other on the left hand side near the bottom, Plate XVII., the wiring of the case being correspondingly divided into two circuits. By this arrangement the temperature all over the case should be nearly uniform and is so far as can be ascertained. To prevent conduction through the metal parts every one of the supporting shafts has a heat insulating collar of vulcanized fibre inserted just inside the temperature case. As a result of these precautions the temperature is maintained more nearly constant than before, although there is a slight fall shown by the prism box thermometer after the heat is first turned on and when the dome temperature is dropping rapidly. This can be prevented from doing any harm by applying the regulation in the afternoon and thoroughly ventilating the dome so that most of the cooling takes place before observing begins.

Flexure

Both instruments have been thoroughly tested for flexure, this being effected by clamping the telescope with spectrograph attached in the meridian and swinging in declination about 180°,

from horizon to horizon. This evidently gives the maximum flexure as in one position the camera is above, in the other below the collimator. By making, by means of suitable diaphragms, adjacent comparison spectra in the two positions, the lineal displacement can be measured and the kilometre value of the flexure at once obtained.

With the Combined Spectrograph

Maximum Flexure

Three-prism form = ·0025 mm. equivalent to 1·8 km. per second.
One " " = ·055 " " " 100 " " "

Flexure Occurring in Practice

Telescope on equator – For movement from 0 to 4 hours.
Three-prism form = ·0018 mm. equivalent to 1·3 km. per second.
One " " = ·007 " " " 13 " " "

Telescope on equator—For movement from 0 to 2 hrs. in *R. A.*
Three-prism form = Not measurable.
One " " = " "

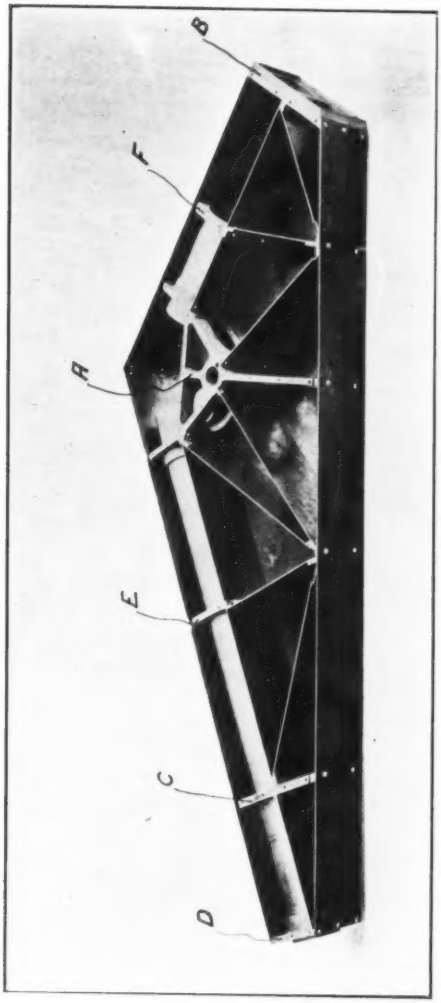
The flexure with three prisms is so small even in its maximum amount that it can be entirely omitted from consideration. Owing to the more extended form of the instrument and to the three-fold greater kilometre value for the same linear displacement the flexure for the single prism is considerable. When we come to the practical use of the spectrograph, say for an exposure of two hours, the flexure becomes unmeasurable around the meridian and not of a very serious amount, if the comparison exposure is properly divided at large hour angles. Although I am satisfied that it is a minimum for that type and weight of instrument, there is no question but that it would be preferable to use an instrument free from flexure. So far as the three-prism spectrograph is concerned, even the maximum flexure is so small in amount as to be entirely compensated for by a proper distribution of the comparison exposures, while the fractional amount remaining in all cases occurring in practice is quite negligible.

The new instrument when subjected to similar tests shows very gratifying results as even the maximum flexure is quite unmeasurable. Three spectra were made on a plate, two to show maximum flexure, and the third with the spectrograph stationary. Although in some lines in the two spectra I thought a slight displacement was noticeable, Mr. Harper, on examining the three with a microscope, was unable to determine in which one the flexure was absent. Consequently one may safely say that the new spectrograph is absolutely free form flexure. This and the practically perfect temperature control should give, so far as these two causes are concerned, thoroughly reliable spectra.

The new instrument has been in use long enough to get a fair estimate of its capabilities and I am glad to be able to say that it fulfils all expectations both in regard to its stability and freedom from temperature troubles and also in the saving of exposure time and consequent increase of output possible by its use. Outside of the evidence furnished by the flexure tests and the constancy of the prism box temperatures, there is no difference whatever in the definition of the longest and shortest exposed spectra. The definition and resolving power with slit width 0·051 mm. are fully equal to those of the former single prism spectrograph at slit width 0·038 mm., while the exposures required with these widths are about 30 per cent. less with the new instrument. A thorough set of tests on the relative errors of measurement in early type stars at different slit widths gives decisive evidence that the maximum accuracy with the new instrument occurs at slit width about 0·05 mm. A width of slit of 0·038 – 0·040 mm. was the widest it was felt safe to use with the previous instrument and the saving in exposure time is consequently over 25 per cent.

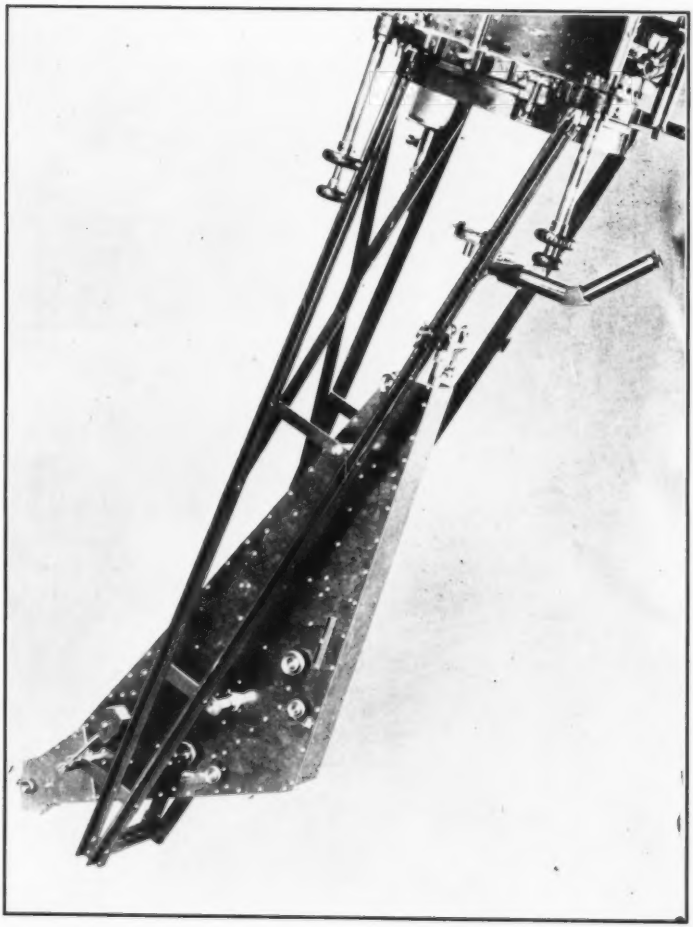
Further the possibility of being able to make the change from one of three prisms or vice versa in a couple of minutes with no disturbance of temperature conditions is a valuable factor in the efficient working of our programme which includes stars requiring both high and low dispersion.

PLATE XVI.



SPECTROGRAPH BOX

PLATE XVII.



THE NEW SINGLE-PRISM SPECTROGRAPH OF THE OTTAWA OBSERVATORY

A great deal of credit for the success of these instruments must be given to Mr. A. S. Mackey, the mechanician of the Observatory, who has constructed them in such a careful and workmanlike manner as to obtain the best possible results from the design. The ease and accuracy of their working bear evidence to his skill.

I am glad to express here to Dr. King, our Director, my appreciation of his willingness to meet the expenses involved, of the interest he has shown and the help and encouragement he has given throughout the whole of this work.

DOMINION OBSERVATORY,
OTTAWA, CANADA,
July, 1909.

TRANSIT WORK AT THE DOMINION OBSERVATORY.

By R. M. Stewart

DURING the longitude season of 1908 observations for clock error at the Observatory were obtained with a field transit on every available night from about the middle of May till the middle of October. The method of observation was that described in a previous article*; a set consisted of seven or eight southern stars well up towards the zenith, combined with three or four north stars between 70° and 80° declination; the telescope was reversed during the observation of each star; two sets were obtained on every night when the weather permitted.

The clock rate being accurately known (its probable variation from day to day being only in the neighborhood of 0·01 sec.) it is possible to compare rigorously the results of two sets taken on any one night, and to use their discordance as a measure of accuracy. If $\triangle$ be the average value of this discordance it is connected with the probable error by the formula

$$r = \cdot 6745 \sqrt{\frac{\pi}{4}} \cdot \triangle = \cdot 5978 \triangle.$$

During the season there were 87 nights on which two determinations of clock error were made, the work being divided among three observers. For one observer the value of $\triangle$ was found to be ·032 sec., for each of the others ·033 sec.; the corresponding value of r is ·020 sec. The value of $\triangle$ found for the old method of observation* was ·039 sec.; as the weight, or efficiency is inversely proportional to the squares of these quantities, the apparent increase in efficiency is 40 per cent.; and presumably this has arisen from the alterations in methods of observation and grouping of stars.

* "Errors of Transit Observations," this Journal July-August, 1908.

It should be mentioned in this connection, that the work during 1908 was perhaps not fully up to standard. The only vial available for the striding level was defective, owing to an accident at the beginning of the season, and consequently considerable trouble was experienced with measurements of level error during the whole summer. In addition, the weather, especially during the latter half of the season, was far from satisfactory on account of the prevailing smoke and haze ; so that, though the number of nights on which observations were possible was above the average, the quality of the work was poorer than usual.

SIMON NEWCOMB

By R. M. Motherwell

THE death of Simon Newcomb on July 11, 1909, removed America's most eminent man of science and one of the world's most famous investigators of theoretical astronomy. Born at Wallace in Nova Scotia, March 12, 1835, he displayed up to the date of his final illness an activity and power for work that may well be classed as marvellous. Famous for his astronomical investigations, it is most fitting that the beginning and ending of his career should be marked by such phenomena as two successive returns of Halley's comet.

Though a Canadian by birth he was of New England descent. His father was a country school teacher and was married at the age of twenty-five to Emily Prince, of Moncton, after searching the country nearly one hundred miles for a suitable helpmeet. Speaking of the marriage his son says: "It was in all respects a happy one, so far as congeniality of nature and mutual regard could go." . . . "My mother was the most profoundly and sincerely religious woman with whom I was ever intimately acquainted, and my father always entertained and expressed the highest admiration for her mental gifts, to which he attributed whatever talents his children might have possessed. The unfitness of her environment to her constitution is the saddest memory of my childhood. More I do not trust myself to say to the public, nor will the reader expect more of me."

His love of books made his boyhood on the whole one of sadness, as it brought forth exclamations of wonder which were distasteful to him, and he regretted that he could never be like other boys. Such books as Fowler's "Phrenology" and Combe's "Constitution of Man" had a profound influence on his mode of life and thought, and afforded him great intellectual pleasure, but he had no one with whom to share this pleasure

and he longed for the skill required in the working of a farm. Had he been able to drive a team of oxen he would have been happy. But his father realized that he would never labor in business and so to fit him for long hours of study he hired him *out* to a farmer during most of their stay at Clements. Gradually he formed a vague conception of a world of light different from the backwoods of Nova Scotia, a world where people devoted themselves to learning and he longed to enter it, but no possibility of doing so presented itself.

When he was sixteen his father was teaching near Yarmouth, N.S., and sent Simon to his grandfather at Moncton, partly to visit and partly because there was no prospect of work at Yarmouth. Shortly after his arrival his grandfather decided to apprentice him to a carpenter, but sickness prevented his doing so, and Dr. Foshay, a renowned physician, was called in. At that time he was in need of an assistant, so an agreement was made whereby Simon was to live with the doctor and prepare his medicines, in return for which the doctor was to instruct him in medicine until he was twenty-one.

He abided by this faithfully for two years and then, realizing that the doctor was a "humbug" and that he was wasting his time, he decided to run away. He left a letter explaining that as the doctor had in no way fulfilled his promises, the agreement was annulled. He was on the road before daybreak and walked until after dark, finally reaching a house where he was "most kindly received." "Thus ended" he says "a day which I always looked back to as the most memorable in my life." After many hardships he at last reached Salem, where his father was teaching and in 1854 he also entered the teaching profession. In 1856 he was teaching near Washington, and so was able to visit the Smithsonian Institution where he found the "greatest treasure" his imigination had ever pictured — the *Meconique Celeste*, by Laplace, translated by Bowditch.

His first attempt at scientific work was a paper in 1854 on "A New Demonstration of the Binomial Theorem." Though lacking in completeness and rigor, part of it was of marked

elegance, such as to bring from Prof. Henry a pleasant note of encouragement to persevere. In 1855 he published a criticism on an article by Evelith who doubted the Copernican system. About this time he met Prof. Henry, who referred him to Mr. Hilgard, of the Coast Survey, and in 1857 he entered "the world of sweetness and light" by being appointed as a computor in the Nautical Almanac Office. Here the system of discipline being such that the work was done mostly when and where the assistant chose, so long as it was done on time, he took advantage of the opportunity by enrolling as student in the Lawrence Scientific School from which he graduated in 1858. From this time the prosecution of the study of exact astronomy occupied the principal part of his time, his rise to the pinnacle of fame in the astronomical world being unchecked.

While at Cambridge he took up the problem of the asteroids, investigating the theory that these bodies had their origin in the shattering of some large planet by collision or explosion. He made an extended examination of the secular variations of the orbits of the asteroids to determine whether at any epoch all the orbits passed through one point. In 1859 he read a paper showing the changes in the orbit of an asteroid for several hundred thousand years, and in 1860 he presented a paper "On the Secular Variations and Mutual Relations of the Orbits of the Asteroids." In it the main conclusion was that, so far as our present theory can show, the orbits had never passed through any common point of intersection. Since then the whole trend of thought and research has been toward the conclusion that the asteroids have been separate bodies since the solar system came into existence.

An appointment to the Naval Observatory in 1861 gave him his first opportunity to engage in observation work, but owing to lack of system very little was accomplished until 1866 when he began work with the new transit circle. It had become evident to him that an error in the right ascension of stars, so that the stars in opposite quarters of the heavens would not agree, might have crept into nearly all modern observation. To discover this

error it was necessary where possible to continue the observations through the greater part of the twenty-four hours. The close of 1869 saw this completed and the error was proved to be real, but owing to certain defects in the instrument and its mounting, he deemed it wiser to turn his attention to the motion of the Moon, and observations of Sun, Moon, and planets became his specialty at the Naval Observatory.

Tables of the Moon's motion had been computed by Hansen from 1750 and an investigation revealed the fact that the Moon was deviating from the predicted course. The possibility of using occultations to determine the Moon's path and motion occurred to him and he found that such observations had been made as early as 1660. A careful reduction, (a task lasting three or four years), of all available observations extended the Moon's history back to 1675 and shewed Hansen's tables to deviate to a surprising extent. This latter work was carried on in Paris during the reign of the Commune, when the city was beseiged by the national forces. Just before the work was completed he was tendered the position of director of Harvard Observatory, but declined, feeling that the Government required his services and that in time the scientific and literary side must come into touch with the political side.

In 1877 came "the happiest change" in his life when he took charge of the Nautical Almanac Office. Here he was able to work with the assurance of being able to carry out his plans, and devoted his energies to the task of improving the fundamental constants employed. The programme which he mapped out involved a discussion of all the observations of value on the positions of the Sun, Moon, planets and brighter stars, made since 1750. The magnitude of this necessitated his confining his attention chiefly to a correction of the reductions already published. In all his work in this office he sought ever to improve the process of calculation as well as the fundamental materials on which the calculations were based. His position being that of director, he was able to hold also a professorship in Mathematics and Astronomy in John Hopkins University from 1884 to 1894,

and in 1897 his term of office in the Almanac Office expired by the time limit.

Although Newcomb was a man of unremitting labor in his official duties, yet he found time to publish a number of books of great value to the general public. His *Popular Astronomy*, published in 1878, is universally recognized as a work of lofty thought. *The Stars, a Study of the Universe*, 1901; *Astronomy for Everybody*, 1903; *Spherical Astronomy*, 1906, and text books on astronomical and mathematical subjects as well as treatises on political economy are numbered among his publications.

When we consider that he was a self-taught man and one of the greatest of scientists we realize what indomitable industry must have characterized his entire life. He was an indefatigable worker, yet he was always ready to lay aside his work and render assistance to his visitors. Such achievements in science coupled with persevereance and increasing labor had their reward in the bestowal upon him all the honours which the world could bestow on a man of science. Among these honours was that of one of the Eight Foreign Associates of the Institute of France and gold medallist of the Royal Astronomical Society of London. In 1904 he was awarded the degree of LL.D. by the University of Toronto.

After a long illness he died, as already stated, on July 11, 1909, and with his death passes out the most conspicuous of American Astronomers. His habitual loftiness of thought, nobility of character, dignified courtesy, and ever-ready helpfulness have endeared him in the the hearts of all his friends, and his life will long be an incentive to harder and more accurate work on the part of all Astronomers.

DOMINION ASTRONOMICAL
OBSERVATORY, OTTAWA,
August 24, 1909.

NOTE REGARDING THE PORTRAIT OF PROFESSOR NEWCOMB.

The writer enjoyed the highly-esteemed privilege of spending the summer of 1907 at the Lick Observatory, and had the

pleasure of meeting some of the distinguished visitors to the famous institution. Among these was Professor Newcomb, who, after delivering a course of lectures at the summer session of the University of California, at Berkeley, came with Mrs. Newcomb to spend a week at Mt. Hamilton. Though he had been a very important adviser in the founding of the Observatory, I believe this was his first visit to it. Of course he was given all the privileges of the place, and every part was shown to him.

He spent much of the time in the fine library, quietly working at the scientific problems upon which he was then engaged—chiefly connected with probabilities and least squares, I think.

Professor Newcomb had a somewhat brusque manner, and when in a mixed company was well assisted by his amiable and well-informed wife. One evening he dined at the "Senior Mess" in the "Brick House," where Professor Tucker was host. After dinner some of the wonders of the world were being discussed, and Professor Newcomb was asked if he had ever seen the Grand Canyon of the Colorado. "No," he replied. "I have never desired to; there is so much *brag* about it."

I had for some days wished to secure a photograph of the great astronomer, but was somewhat diffident of asking the privilege, fearing I might receive a very short reply in the negative. But the day after our dinner, when he was in the library, I summoned up sufficient courage to ask him to sit before my folding kodak. I found my fears were entirely groundless, as he met my request with the greatest amiability. Placing the camera on a table and simply focussing by scale, I made two exposures, and obtained two excellent negatives.

A day or two later the visitors bade good-bye to the entire staff gathered at the front entrance of the Observatory. Professor Newcomb stated that though he was said to be somewhat cold and undemonstrative he must express his high appreciation of the kind treatment he had received.

On making some prints from my negatives I sent one to Professor Newcomb at Washington, and in reply received a visiting card with the inscription, "Thanks for photo; hoping that we may meet again."

C. A. C.

MEETINGS OF THE SOCIETY

At Peterborough

On the evening of June 22 Hon. J. R. Stratton, M. P., and Mrs. Stratton entertained the members of the Peterborough Centre of the Astronomical Society of Canada, at their residence "Strathormond." The grounds were beautifully illuminated by suspended electric lights, while convenient seats and rugs were distributed in different parts of the lawn.

Three telescopes were placed on the grounds for the use of those in attendance, and much pleasure was derived from them.

Among the guests in attendance from outside points were the Society's Honorary President, Dr. King, C. M. G., of Ottawa, and Dr. C. M. Stratton, of Napanee.

A very pleasant evening was spent in viewing the heavenly bodies through the telescopes, and in social intercourse. In addition the 57th Regimental Band provided music, and refreshments were served from a marquee on the lawn, Coleman Bros. catering in a creditable manner.

E. F.

NOTES FROM THE DOMINION OBSERVATORY

Astrophysics

It was announced in these notes a year or so ago that the Spectroscopic binary star B.D. $-1°$, 1004 had a velocity curve very similar to that of *ι Orionis*. Its orbit has recently been determined by Mr. Harper with the result of increasing the similarity before noted in the velocity curves and elements. It has a marked secondary disturbance of the same period as the primary. A similar secondary effect was suspected in *ι Orionis* when its orbit was determined but, owing to the poor quality of the spectrum, it was not introduced in the solution. The work on B.D. $-1°$, 1004 led to a further determination, by the writer, of the elements of *ι Orionis*, giving a secondary disturbance of the same character. The similarity between the velocity curves and orbits of these two binaries is so striking as, taken in conjunction with the facts that they are of the same spectral type and within 5° of one another in the sky, to lead one to think it must be more than a coincidence although no physical cause for such similarity is known.

A paper by Messrs. Plaskett and Harper on these two stars was presented by Dr. King at the Winnipeg meeting of the British Association for the Advancement of Science.

Soon after the new single prism spectrograph was brought into use it was seen that the spectra obtained were, even considering the increased absorption of the larger prism, too weak in the violet. Experiments were hence undertaken to determine the cause and to find, if possible, a remedy. They showed that no marked improvement was produced when the form of the color curve was changed, this being effected by changing the

distance between correcting lens and slit. It was also shown not to be due to poor adjustment of the guiding apparatus. The cause was finally located in imperfect collimation of the correcting lens. A slight displacement was found to disperse the star image, the position of the image in violet light being to one side of that in blue-green light. As the latter is of much better visual intensity and will consequently be kept central on the slit, it is possible that the violet image may be entirely off the slit causing the weakness noticed. The experiments further showed that, in different positions of the telescope, the changes in collimation due to flexure, combined with the dispersive effect of the atmosphere, gave noticeably different arrangements of the composite image. It is hence necessary for the best results to so adjust the collimation of the correcting lens, in each position of the telescope, that the dispersion thereby produced may be exactly equal and opposite to that given by the atmosphere, the latter evidently varying with the zenith distance. Such an arrangement will shortly be applied here. A paper on this subject was given at the Williams Bay Meeting of the Astronomical and Astrophysical Society of America.

For some time past Mr. Motherwell has been investigating the field and the aberrations of the 8-inch Brashear doublet used for stellar photography. It was shown to have negative aberration of about 3·5 mm. and this was deemed sufficient to explain the halo observed around the star images formed by it. Through the kindness of the J. A. Brashear Co. the lens was recently refigured and a test has shown that the aberration is now negligibly small. The images now given show no evidence of halo and are very small and sharp. In addition the field is considerably flatter than ordinarily given by this type of objective, and consequently it satisfactorily covers a larger extent of sky than usual. Its attachment to the tube of the equatorial telescope, which must hence be employed for guiding whenever the camera is used considerably limits its usefulness and it is hoped that a separate mounting may soon be obtained.

J. S. P.

NOTES FROM THE METEOROLOGICAL SERVICE

Earthquake Records by Milne's Seismograph

TORONTO

R. F. Stupart, Director.

P.T. = Preliminary Tremors. L.W. = Large Waves. Time is Greenwich Civil Mean Time, 0 or 24 H = midnight.

No.	Date 1909	P.T Comm.	L.W. Commence	Max.	End.	Max. Amp.	Durat.	Remarks
		h m	h m	h m	h m	mm.	h m	
858	June 3	19 21·5	19 49·0	20 8·1	21 19·0	1·6	1 57·5	Sumatra.
859	" 8	No P.T's	6 7·7		8 11·0	0·7	2 3·3	India.
860	" 27	7 45·0	8 16·0	8 40·0	9 18·8	0·5	1 33·8	
861	July 7	11 2 0			11 15·5	0·1	0 13·5	
862	" 7	21 57·0	22 1·0	22 30·0	23 28·5	1·1	1 31·5	India.
863	" 30	10 58·2	{11 4·0} {11 12·5}	11 23·0	13 40·0	5·0	2 41·8	Medium. Mexico.
864	" 30	14 7·0			14 22 0	0·1	0 15·0	
865	" 31	19 30·0	16 38·2	19 41·5	21 16·0	2·1	1 40·0	Mexico.

Angular value 1 mm. = 0″·70

VICTORIA, B. C.

E. B. Reed, Superintendent.

No.	Date 1909	P.T. Comm.	L.W. Comm.	Max.	End	Max. Amp.	Durat.	Remarks
		h m	h m	h m	h m	mm.	h m	
881	May 23	6 2·8			6 21·8	0·2	0 19·0	[nois-
882	June 3	19 10·0	19 50 2	20 6·5	21 16·0?	1·6	2 6·0?	Marked thickenings, Illi- Sumatra: active and ex-
883	" 8	6 0·0		6 45·4	7 38·5?	1·0	1 38·5?	Extended. India. Itended.
884	" 10	8 6·3			8 12·3	0·3	0 6·0	
885	" 27	7 38·2			8 55·0	0·4	1 16·8	
886	July 7	21 53·0	22 3·0	22 25·3	23 22·0	1·5	1 29·0	Small. India.
887	" 21	4 16·0			4 22·5	0·2	0 6·5	
888	" 30	10 59·4	11 11·4	{11 13·3} {11 27·7}	13 31·0	20+	2 31·6	Very large. Double Max [across paper.
889	" 30	14 4·8		14 6·0	14 18·2	1·8	0 13·4	
890	" 31	19 31·3	19 38·7	19 43·4	Lost	4·5	?	Fairly large. Mexico.

Angular value 1 mm. = 0″·76.

Summary Report of the Weather in Canada

June, 1909

Temperature.—The temperature was generally and uniformly a little above the average throughout the Dominion, the positive departure in nearly all districts varying from 1 to 2 degrees, although in isolated localities it was as much as 3 degrees.

Precipitation.—The rainfall was very deficient in nearly all portions of the Dominion, except in a few isolated localities, noticeably in the Gaspe Peninsula, and in the extreme south-western portion of Saskatchewan, where for the most part the usual quantity appears to have been well exceeded. Ottawa City recorded an amount in excess of the average, also a few places in the extreme south-western counties of Ontario, in each instance owing to the prevalence of thunderstorms in the localities affected. In British Columbia the negative departure varied from six-tenths of an inch to 3 inches. In the Western Provinces, the deficit was very generally from 33 to over 100 per cent., in Ontario, from 1¼ to 2½ inches, in Quebec, from ¾ of an inch to over 2 inches, and in the Maritime Provinces from ½ to nearly 3 inches.

July, 1909

Temperature.—The temperature of July was below average over the major portion of the Dominion, the largest negative departures, amounting to 3°, being in the mainland of British Columbia and in the peninsula of Ontario. The average was, however, somewhat exceeded in Manitoba, Alberta, Northeastern Saskatchewan, the Rainy River and Thunder Bay districts of Ontario, and very locally in Nova Scotia, the largest positive departures reported being 4° at Prince Albert, and 2° at Winnipeg and Minnedosa.

Precipitation.—A marked feature of the month was the excessive precipitation over the Western Provinces, where in many districts the rainfall was as much as three times the normal amount; Qu'Appelle recorded 7·20 inches, Moose Jaw 6·50 inches, Swift Current and Medicine Hat 4·70 inches, and Calgary

Temperatures for June and July

Station	June		July	
	Highest	Lowest	Highest	Lowest
Yukon				
Dawson	84	29	82	36
British Columbia				
Atlin	76	30	76	38
Agassiz	86	39	87	40
Barkerville	76	28	72	34
Kamloops	98	44	96	46
New Westminster	76	42	80	42
Port Simpson	66	38	68	46
Vancouver	78	42	77	46
Victoria	76	42	78	45
Western Provinces				
Battleford	86	44	86	48
Broadview			95	40
Calgary	82	36	83	41
Carman			95	40
Edmonton	82	35	83	39
Medicine Hat	90	44	92	41
Minnedosa	90	32	86	42
Moosejaw	91	33	93	46
Oak Bank				
Portage la Prairie			83	48
Prince Albert	86	32	84	46
Qu'Appelle	82	32	84	44
Regina				
Swift Current	88	38	88	46
Winnipeg	90	34	87	44
Ontario				
Agincourt			86	44
Aurora	89	37	89	43
Bancroft			85	48
Barry			86	48
Beatrice	85	38		
Birnam	85	41	88	42
Bloomfield			85	48
Brantford			90	44
Bruce Mines	90	36	86	40
Chatham	87	34	85	45
Clinton	85	37	87	39
Cottam				
Cockburn Island			89	43
East Toronto			91	45
Gravenhurst	87	36		
Haliburton			86	36
Hamilton			90	50
Huntsville	84	33	84	44
Kenora				
Kinmouth			82	36
Kingston	79	41	80	47
Lakefield			92	49
London				
Lucknow	86	34	87	40
Madoc			88	43
Meaford				
Midland	90	45		
North Gower				
Otonabee	89	42		
Ottawa	86	41	85	47
Owen Sound	90	36	91	45
Paris	89	41	92	45
Parry Sound	87	45	89	44
Peterboro'			87	47
Point Clark	90	35		
Port Arthur	84	33	84	43
Port Burwell	80	41	83	44
Port Dover	83	41	84	46
Port Hope				
Port Stanley	84	40	83	42
Ronville	85	34		
Sarnia			89	48
Southampton	88	38	86	46
Stonecliffe	92	32	88	40
Stony Creek	90	44	93	48
Stratford			86	43
Toronto	90	43	87	48
Uxbridge	89	39	89	45
Wallaceburg				
Welland			89	44
White River	88	27	82	35
Quebec				
Brome	84	31	86	39
Father Point	76	32	77	43
Montreal	86	44	85	52
Quebec	85	38	85	46
Sherbrooke	85	35		
Maritime Provinces				
Charlottetown	87	37	84	50
Chatham	87	34	85	45
Dalhousie	82	25	83	39
Fredericton	89	29	88	44
Halifax	94	36	88	45
Moncton	90	33		
St. John	79	39	80	47
St. Stephen	90	25		
Summerside			82	44
Sussex			88	40
Sydney	86	32	86	39
Yarmouth	72	37	79	44

4.10 inches. The normal was also exceeded over the larger part of Ontario and especially in Rainy River, Thunder Bay districts and all the more northern portions of the Province; but a slight deficiency occurred near Lake Erie and in the Ottawa Valley. In Quebec and the Maritime Provinces the departure from average was generally small and in most localities negative. In British Columbia departures from average were mostly in excess.

BOOK REVIEWS

Spectroscopie Astronomique par P. Salet, Astronome a l'Observatoire de Paris. 18mo., 440pp., cloth, 5 francs. Paris; O. Doin & Fils, 1909.

This work on Astronomical Spectroscopy is one of the volumes of the great Scientific Encyclopedia which is now being published, and which is to comprise forty divisions and about one thousand volumes. In the division devoted to astronomy and celestial physics there will be twenty-nine volumes.

There is certainly room for such a work as this, as at present there exists but one treatise on the subject, that of Scheiner, which was published almost twenty years ago. Since then great progress has been made in this branch of science, and a book in English giving a comprehensive survey of the subject would surely have an extensive circulation.

In the introduction of twenty pages M. Salet gives an admirable review of the whole subject, with a discussion of the relation of astro-spectroscopy to cosmogony and philosophy.

Then follow chapters on spectroscopic apparatus of various kinds, including a clear explanation of the spectroheliograph and the interference spectroscope; on the Doppler effect and its numerous applications; on spectra of the Sun, discussing its various features in detail and including an account of Hale's discovery of a magnetic field; on the spectra of the planets and of the Zodiacal light; on the spectra of comets; and finally on the spectra of stars and nebulæ.

Although the book is intended for the general reader there is an original and pretty complete mathematical treatment of the Doppler principle, and also a discussion of the correction required to be made to observations of radial velocities. The classifica-

tion of the stars is also discussed at length. After reviewing the schemes given by Secchi, Vogel, Lockyer, Pickering, Huggins and others, the author gives a classification into five types which he calls helium, hydrogen, solar, titanium and carbon stars.

Another valuable feature is the bibliographical references appended to each chapter.

C. A. C.

Tafeln für Numerisches Rechnen mit Maschinen, herausgegeben von O. Lohse; 4to, pp. 124, Leipzig, W. Engelmann, 1909.

The tables which comprise this volume were compiled by Professor Lohse in order to facilitate the use of calculating machines in mathematical computation.

They are as follows: reciprocals of the numbers from 1 to 5000; trigonometrical functions for each hundredth of a degree; squares of numbers from 1 to 1000; table for calculating square roots; formulas in plane and spherical trigonometry; formulas from the differential calculus; tables for transforming time and arc measurements; and a table of wave-lengths of iron.

The book is well printed on heavy paper and will be found very useful in extending the applications of computing machines.

C. A. C.

Observations Méridiennes. By Dr. F. Boquet, of the Observatory of Paris. Paris, O. Doin & Fils, 1909. 2 vols., 5 francs each.

This is a publication which, without doubt, will speedily find its way into the libraries of astronomical institutions. It aims at giving a fairly complete account of the present state of the science of meridian astronomy, and in that direction fulfils a long-felt want.

Of the two volumes comprised, the first deals with instruments and methods of observation, one division dealing with transit observations and another with zenith distances. After a full discussion of the different parts of the telescope itself it describes in detail the latest and most improved forms of auxiliary instruments, such as clocks, chronographs, self-registering micro-

meters, collimators, artificial horizons and azimuth marks. It also fully discusses methods of observation and reduction of observations to the meridian of the instrument.

The second volume deals with the corrections for instrumental and personal errors and for refraction. This discussion of personal equation, which, together with the description of the instruments, forms the most useful and important part of the book, is very full and thorough, and shows a good grasp of all the ramifications of this greatest bugbear of observation.

If the book has a fault, it is the quite natural and probably unavoidable one of giving, perhaps, too much preponderance to the description of work and instruments of French origin, and the omission or curtailment of description of foreign appliances. Copious references are given to original memoirs in footnotes throughout the text ; at the end of the second volume is a bibliographic index which alone is worth the price of the two volumes.

R. M. S.

Scientific Ideas of To-Day, Popularly Explained. By Charles R. Gibson. London : Seeley & Co. ; 1909.

In a neat and well-printed volume of some three hundred pages, the author has brought together the more outstanding results of the newer investigations in physics and chemistry, has explained them popularly yet scientifically, and has discussed them in relation to such matters as the evolution of the stars, the age of the earth, the nature of gravitation. Incidentally is considered sufficient of the elementary theory of light, electricity and magnetism to allow the intelligent reader, untrained scientifically though he may be, to appreciate what is said about radioactivity and its bearing on the ultimate theory of matter. In the appendices are useful lists and tables as well as important notes.

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ASTRONOMICAL NOTES

DR. LOWELL'S 40-INCH REFLECTOR.—The Alvan Clark and Sons Corporation of Cambridge, Mass., is just completing for the Lowell Observatory, Flagstaff, Arizona, a reflecting telescope of 40 inches aperture and 18 feet 4 inches focal length. The mirror for Dr. Lowell's telescope is of plate glass from the works of St. Gobain, the thickness of which is 7 inches and the weight 907 pounds. It is thus much thicker than Common's glass. The thickness is to insure stability. The cell weighs 915 pounds and consists of an iron ring with zinc blocks at intervals between it and the glass, so proportioned that the combined coefficient of expansion of the two is the same as that of the glass. The latter can thus be held in place without risk of distortion through strain in the cell. The mirror rests on nine three-point lever-supports, arranged so that the weight is supported uniformly.

The telescope is so constructed that it can be used either for planetary photography or for photographing nebulæ and stars. For the former purpose it is arranged as a Cassegrain with secondary mirrors giving it a focal length of 154 feet and 75 feet respectively. With the 75-foot mirror an amplifying lens can be introduced as in a refractor. By this means the efficiency of a reflector on planetary photography as compared with a refractor can be directly tested, and also the combination of a reflector with an amplifying lens in the same way. By a flat, the cone of rays is diverted out at right angles to the tube to the camera. For stars and nebulæ the telescope is arranged as a Newtonian, the images being photographed in the principal focus.

The housing of the telescope is effected by sinking a well six feet into the ground, over which is placed a hemispherical dome of wood and canvas. The sinking of the floor accomplishes two

objects: 1, it shields the mirror more successfully from wind; and 2, it tends to equalize the temperature, an important matter in preventing the deformation of the image through expansion or contraction of the glass or tube.

The construction is specially designed to secure greater detail in planetary definitions than has been hitherto obtained, through the clear and steady air at Flagstaff.

In reference to condensations noted on Saturn's rings some little time ago when the rings were edgewise to us. Professor Barnard, in *Monthly Notices* R. A. S., concludes "That they were due in some manner to the transmission of the sunlight through the rings (through the crape ring and Cassini division, perhaps) is further shown by the fact that no trace of them was seen when the bright side of the rings was presented to us at a similar low angle to that of the dark side when they were visible."

A dispatch from Harvard, dated September 15, states that Professor W. W. Campbell and Albrecht of the Lick Observatory, compared the spectra of Mars and the Moon on Mount Whitney, September 1 and 2, 1909; a little water vapor. Bands estimated equal intencities, and very faint. Zenith distance 42°. Barometer 17·72 inches. Air temperature – 17°·8. Fahr. Wet Thermometer – 22°·2 Fahr.

(These observations were made at the time of occultation of Mars by the Moon, Sept. 1. The water vapor indicated by Moon's spectrum is very slight).

A dispatch from Harvard College Observatory dated September 13, states: "Halley's Comet was observed by Professor Wolf, at Heidelberg, September 11, 5642, Greenwich Mean Time in R. A. $6^h\ 18^m\ 12^s$, Dec. + 17° 11′. Visible in a large telescope.

A dispatch dated September 20, states:—A letter has been received from Professor E. B. Frost, Director of the Yerkes Observatory, stating that Halley's comet was observed visually by Professor S. W. Burnham with the 40-inch telescope, on Sept. $15^d\ 21^h\ 39^m$ G. M. T., in App. R. A. $6^h\ 18^m\ 51^s{\cdot}1$ and App. Dec.

+17° 9′ 44″. The comet followed B. D. +17° 1232 by 12″·7, North 4′ 12″·1. The comet was also photographed with the 2-foot reflector, on Sept. 15 and 16, by Mr. Oliver J. Lee.

A second letter from Professor Frost, states that the comet was also observed visually by Professor E. E. Barnard, on Sept. 17^{d} 21^{h} 1^{m} 30^{s} G. M. T., in App. R. A. 6^{h} 19^{m} 0^{s}·90 and App. Dec. +17° 9′ 0″·8. The comet followed A. G. 2122 (=+17° 1232) by 0′ 22″·55, North 3′ 28″·9.

"Description: 15½ mag., 12″ diameter, with possibly a faint nucleus or indefinite fleck of light in it." The comet was also photographed by Mr. Lee at the same time.

Sir J. J. Thomson in his presidential address before the British Association, for the Advancement of Science, at Winnipeg, in speaking of the æther said:—"The æther is not a fantastic creation of the speculative philosopher; it as essential to us as the air we breathe. For we must remember that we on this Earth are not living on our own resources; we are dependent from minute to minute upon what we are getting from the Sun, and the gifts of the Sun are conveyed to us by the æther. It is to the Sun that we owe, not merely night and day, springtime and harvest, but it is the energy of the Sun, stored up in coal, in water-falls, in food, that practically does all the work of the world.

How great is the supply the Sun lavishes upon us becomes clear when we consider that the heat received by the Earth under a high Sun and a clear sky is equivalent, according to the measurements of Langley, to about 7000 horse-power per acre. Though our engineers have not yet discovered how to utilize this enormous supply of power, they will, I have not the slightest doubt, ultimately succeed in doing so; and when coal is exhausted and our water-power inadequate, it may be that this is the source from which we shall derive the energy necessary for the world's work. When that comes about, our centres of industrial activity may perhaps be transferred to the burning deserts of the Saraha, and the value of land determined by its suitability for the reception of traps to catch sunbeams."

J. R. C.

ANCIENT APPARITIONS OF HALLEY'S COMET

The years given by Dr. A. D. Watson for ancient apparitions of Halley's Comet (this JOURNAL, p. 212) appear to be erroneous. Hind has a table of the observed returns before the Conquest in his book on "Comets," (1852), and Crommelin gives the early perihelia as under :—

B. C.	240—May 15.	A. D.	451—July 3.
"	163—May 20.	"	530—Nov. 15.
"	87—Aug. 15.	"	607—Mar. 26.
"	12—Oct. 8.	"	684—Nov. 26.
A. D.	66—Jan. 26.	"	760—June 10.
"	141—Mar. 25.	"	837—Feb. 25.
"	218—Apr. 6.	"	912—July 19.
"	295—Apr. 7.	"	989—Sept. 15.
"	373—Nov. 7.		

44 Egerton Road,
Bishopston, Bristol,
England.

W. F. DENNING.

NOTE IN REPLY TO W. F. DENNING.

On page 212 of last issue, referring to dates given for the apparitions of Halley and formerly accepted, the article reads:

"Up to this point, as will be seen by the irregularity of the periods, the records are more or less unreliable."

These early dates were presented as interesting tradition, not as science. Hind's and Crommelin's work on this subject is the most valuable in existence, yet the reader should remember that no records are to be accepted as final that are not the results of actual observation. The pre-Norman dates are no doubt partly the result of records made by contemporary astronomers, but they are necessarily to a considerable extent the result of computations made by the astronomers whose names are here mentioned.

The author of the article is much indebted to the writer of the foregoing note.

A. D. W.

NOTES AND QUERIES

Communications are Invited, Especially from Amateurs. The Editor will try to Secure Answers to Queries.

Queries

1. Why is it that Mars is well placed for observation only once in two years ?—E. E. G.

Answer

This can best be explained by means of a diagram. With the same centre draw two circles, one with radius one inch, to represent the earth's orbit, the other with radius one-and-a-half inches to represent that of Mars. The Sun, of course, will be at the centre.

Now imagine yourself at the Sun and observing the motion of these two planets. The earth will be seen to revolve in 365 days and Mars in 687 days. These are their sidereal periods.

In order to observe Mars best, the Sun, the earth and Mars should be in a straight line, with the Sun and Mars on opposite sides of the earth. In this case Mars is said to be *in opposition.*

Let us start with an opposition. Then if Mars stood still the earth would complete its revolution in 365 days and there would be an opposition again. But Mars does not stand still ; it is moving all the time, and so when the earth has come around to the same place Mars has moved on in its orbit. Indeed, by the time the earth has made a second revolution it will not have caught up, but it must go on about two months longer before it comes abreast of Mars. Thus from one opposition to the next the interval is about 2 years, 2 months.

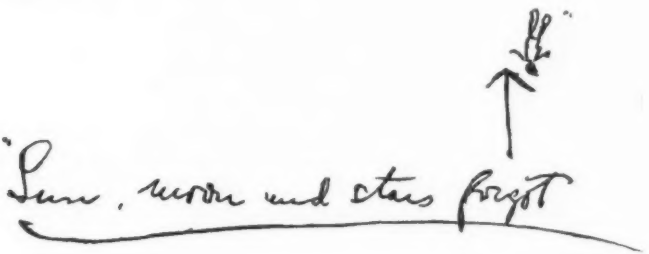
The last opposition was on July 6, 1907 ; the next will be on September 24, 1909.

Some oppositions are more favorable than others. This arises from the fact that the orbits of the earth and Mars are not exact circles, but ellipses and so these curves will be nearer to-

gether at some places than at others. The opposition this year will be a favorable one. For a chart of the stars giving the position of Mars during the year 1909, see the January-February issue.

C. A. C.

At a recent examination in elementary astronomy one of the candidates after writing a pretty unsatisfactory paper ended as follows :—



C.

At a recent examination the following answers were given :

Define Declination, Right Ascension, Latitude, Longitude, Refraction, Aberration and Dip.

"Aberration is an error on account of the principle of optics causing a difference between true and apparent longitude of sun."

"Aberration is a distortion of the outline of an object caused by conditions of the atmosphere."

"Dip is the circle that the pole of the earth describes in 33 years, by it moving."

"Dip is the elevation of the observer below the horizon."

What is an Axiom, Theorem, Proposition, Problem, Hypothesis ?

"Hypothesis is the side in a right-angled triangle opposite the right angle." K.

PREDICTIONS FOR SEPTEMBER AND OCTOBER

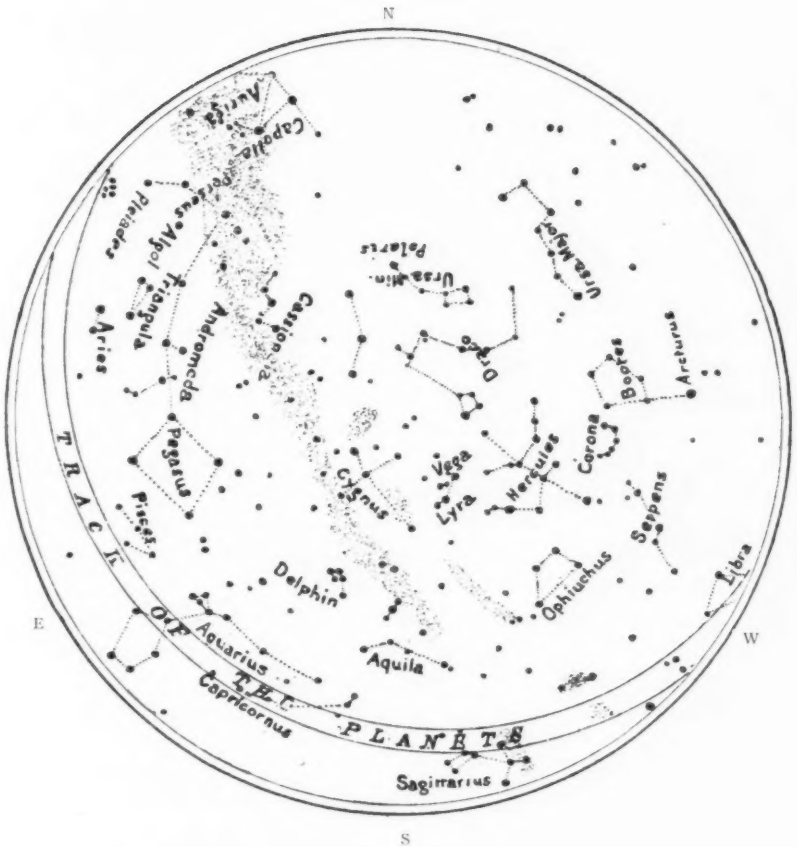
PREPARED BY R. M. STEWART

Constellations on the Meridian in September

Cygnus (The Swan) is marked by five stars forming a conspicuous cross in the heavens, which may, without unduly stretching the imagination, be likened to the outline of a flying swan. It is in the Milky Way, which here begins to separate into two streams, and contains telescopic fields of great magnificence. Its brightest star α, sometimes known as Arided or Deneb, crosses the meridian two hours and five minutes after Vega and a few degrees higher, almost exactly in the zenith; it is between the first and second magnitudes, but has no appreciable parallax or proper motion, being, therefore, at an immense distance, and possibly surpassing Vega or even Sirius in size; it is approaching us at the rate of about forty miles per second. About 15° east of α there suddenly appeared, in 1876, a Nova of the 3rd magnitude, which later faded irregularly to the 14th magnitude.

Vulpecula (The Fox) and *Sagitta* (The Arrow) are two small constellations immediately south of Cygnus, between it and Aquila (See map). Neither of them contains any bright stars, but as both are traversed by the Galaxy the telescopic fields are good. Vulpecula, in particular, contains one of the prettiest of telescopic objects, the well-known Dumb-Bell Nebula. Sagitta is one of the ancient constellations, but Vulpecula is comparatively modern, having been named by Hevelius about the end of the seventeenth century.

Delphinus (The Dolphin), otherwise known as Job's Coffin, is another small constellation to the immediate north-east of Aquila, containing a little group of five stars of the third magnitude. Two of these bear the not too euphonious names of



THE CONSTELLATONS ON SEPTEMBER 15, AT 8 P.M.

Sualocin and Rotanev; it has been conjectured that they were derived by reversing the letters in the name (Nicolaus Venator) of an assistant at the Palermo Observatory, in the catalogue emanating from which these stars are so denominated.

Aquila (The Eagle) is on the meridian about nine o'clock at the beginning of September, being then about half way from the horizon to the zenith. It is conspicuously marked by Altair, a fine star of the first magnitude, which crosses the meridian seventy minutes after Vega. Though Aquila is a large constellation it contains only three other moderately bright stars, all of the third magnitude.

Capricornus (The Goat), the tenth sign and eleventh constellation of the zodiac, contains four stars of the third magnitude and four of the fourth. It may be readily recognized by two stars pointing directly to Altair, which pass the meridian twenty-seven minutes after him, about 20° lower; just below them in the same line is a fourth magnitude star; four other stars, the first two of the fourth, the others of the third magnitude, extend in a line almost at right angles to the former towards the east.

CONSTELLATION ON THE MERIDIAN IN OCTOBER

Aquarius (The Waterman), a large and irregularly shaped constellation, lies to the east and north of Capricornus. It is the eleventh sign and twelfth constellation of the zodiac, and is occupied by the Sun from the middle of February till the middle of March; it contains seven third magnitude and eight fourth magnitude stars. It is not conspicuous, but if attentively examined the stars in the south-eastern part of it will be found to have a trend downwards, which, doubtless, gave occasion to the idea of water flowing from a jar.

Piscis Australis (The Southern Fish), which is not to be confounded with the zodiac constellation of Pisces, lies to the south of Aquarius and Capricornus. Its brightest star, Fomalhaut, is the most southerly of the first magnitude stars visible in these latitudes; it is on the meridian at nine o'clock on the 20th of October, when it is only about 15° above the southern horizon.

Cepheus, one of the polar constellations, extends northward to the pole between Draco and Cassiopeia, and southward as far as Cygnus. Though a large constellation, it contains only three stars of the third magnitude and four of the fourth; however, it atones for this by the comparatively large number of interesting double and variable stars, several of the latter being of quite short period.

Mercury

Mercury reaches its greatest elongation east on September 17th; owing, however, to its large southern declination it sets only about twenty-five minutes after the Sun; it continues to be an evening star till October 12, when it is in conjunction with the Sun. It reaches its greatest elongation west on October 28, and at this time rises about five o'clock, or over an hour and a half previous to sunrise.

Venus

Venus continues to become increasingly conspicuous in the evening sky during September and October as it increases its distance from the Sun. At the beginning of September it is about 30°, and by the end of October over 40°, to the eastward of the latter. It is in conjunction with the Moon on September 17 and October 17, being on both occasions some distance to the south.

Mars

In favorably situated observatories Mars is now the object of careful scrutiny. It will be in opposition to the Sun on September 24, though its closest approach to the earth will have been some six days earlier. This is by reason of the eccentricity of its orbit, which is also the cause of the present opposition being one of the most favorable. On September 18 it will be distant from the earth some 36 millions of miles; at the last opposition, in 1907, which was also a comparatively favorable one, the distance was nearly two millions of miles greater; if the present opposition had occurred about a month earlier the distance would have been further diminished by roughly a million

miles. During September and the greater part of October its motion will be retrograde, but on October 26 it again begins to move slowly towards the east. It is occulted by the Moon on the evening of September 1 ; this will, however, have occurred before this issue of the JOURNAL goes to press. The Moon is again interposed between it and the earth on September 28, but the occultation is not visible in Canada.

JUPITER

On account of its proximity to the Sun Jupiter will be invisible during September, being in conjunction on September 18. By the beginning of October it will rise an hour, and by the end nearly three hours, before the Sun, and will be visible as a morning star. It is in conjunction with the Moon on October 12.

SATURN

Saturn, which has been retrograding since August 5, continues to do so during September and October, its motion among the stars being about 4° to the westward. It is in opposition to the Sun on October 13, and during this whole period will be favorably situated for observation. It is in conjunction with the Moon on September 2, September 30, and October 27th, being on each occasion somewhat to the north.

SOME INTERESTING DOUBLES VISIBLE IN SMALL TELESCOPES

β Cygni. R. A. $19^h\ 27{\cdot}1^m$; Dec. 27° 46′ ; magnitudes 3,5·3 ; distance 34″·3 ; colors yellow and blue ; the brighter has been suspected of variability ; the spectra of the components are complementary. It is a beautiful object, affording a fine contrast of colors.

Σ 2538. R. A. $19^h\ 28{\cdot}1^m$; Dec. 36° 30′ ; magnitudes 8·2, 8·3 ; distance 53″ ; 8·3 has a companion of 8·7 at 6″·1, and 8·2 a companion of 13th mag. at 4″·4 ; also a minute star between the pairs.

δ Cygni. R. A. $19^h\ 42{\cdot}1^m$; Dec. 44° 54′ ; magnitudes 3, 7·9 ; distance 1″·8 ; difficult with moderate apertures because companion falls on interference ring ; hence often visible in twi-

light but invisible in dark sky. Binary of 376·6 years period. Another pair 12′ north, and a triplet 6ᵐ following.

κ Cephei. R. A. 20ʰ 12·0ᵐ ; Dec. 77° 26′ ; magnitudes 4, 8 ; distance 7″·4 ; white and blue.

————. R. A. 20ʰ 45·6ᵐ ; Dec. 32° 53′ ; triplet ; magnitudes 8·7, 9, 10 ; distances 9″·6, 17″·9 ; brightest star very red ; others blue.

61 *Cygni.* R. A. 21ʰ 2·8ᵐ ; Dec. 38° 18′ ; magnitudes 5·3, 5·9 ; distance 15″·3 ; close south of the brighter is a purple star of 10·5 mag. These stars were the first whose distance from the solar system was measured ; only one star (*α* Centauri) is closer.

Σ 2799. R. A. 21ʰ 24·4ᵐ ; Dec. 10° 41′ ; magnitudes 6·6, 6·6 ; distance 1″·4.

13*H. Cephei.* R. A. 21ʰ 36·1ᵐ ; Dec. 57° 05′ ; magnitudes 6·3, 7·9, 8 ; distances 11″·7, 20″ ; also a small close companion to the 6.3.

ε Pegasi. R. A. 21ʰ 39·7ᵐ ; Dec. 9° 27′ ; magnitudes 2·5, 11·5, 8·8 ; distances 81″·4, 140″·3. This object, when near the meridian, will exhibit a phenomenon first noticed by Herschel—the pendulum-like oscillation of a small star in the same vertical with a large one, when the telescope is swung from side to side. This, he thought, was due to the longer time required for a fainter light to affect the retina, so that reversal of motion is first perceived in the brighter object. Same effect is striking in *δ* and *ζ* Orionis and *δ* Herculis.

μ Cygni. R. A. 21ʰ 40·1ᵐ ; Dec. 28° 20′ ; magnitudes 4,5 ; distance 5″·6 ; binary ; observers differ as to colors.

κ Pegasi. R. A. 21ʰ 40·5ᵐ ; Dec. 25° 14′ ; magnitudes 3·9, 10·8 ; distance 11″ ; the brighter star is a close binary, distance ·1″, period about 11 years.

Σ 2840. R. A. 21ʰ 48·9ᵐ ; Dec. 55° 22′ ; magnitudes 6,7 ; distance 20″.

ζ Aquarii. R. A. 22ʰ 24·1ᵐ ; Dec. −0° 29′ ; magnitudes 4, 4·1 ; distance 3″·6 ; binary of period 1624·3 years.

δ Cephei. R. A. $22^h\ 25{\cdot}8^m$; Dec. 57° 57′; magnitudes 4, 5·3; distance 40″·9; yellow and blue; the components have common proper motion; larger star is variable.

8 *Lacertæ.* R. A. $22^h\ 31{\cdot}8^m$; Dec. 39° 10′; four stars, A, B, C, D; magnitudes 6, 6·5, 10·2, 8·5; distances A B 22″·5, B C 28″·2, B D 66″·5; D has also a companion of the 14th magnitude, distant about 10″.

Variable Stars

Name	R. A.		Dec.		Mag.	Period
	h	m	°	′		
R Cygni	19	34·4	50	00	5·9 to 14	425·7 days
N² Cygni	19	47·1	32	41	4–6·5 to 13·5	406 days
10 Sagittæ	19	51·9	16	24	5·6 to 6·4	8d 9h 11m
U Delphini	20	41·3	17	46	6·4 to 7·3	Irregular
X Cygni	20	39·8	35	16	6·4 to 7·2–7·7	16·389 days
T Aquarii	20	45·2	−5	29	6·7 to 13	203·3 days
T Vulpeculæ	20	47·6	27	55	5·5 to 6·5	4·4362 days
Y Cygni	20	48·5	34	19	7·1 to 7·9	1d 11h 57m 22s
R Vulpeculæ	21	00·3	23	27	7·5 to 14	136·9 days
T Cephei	21	08·3	68	07	5·2–6·8 to 9·5–9·9	383·3 days
W Cygni	21	32·6	44	58	5 to 6·7	130·8 days
S Cephei	21	36	78	13	7·4 to 11·5	484 days
μ Cephei	21	40·7	58	22	4 to 5	Irregular
δ Cephei	22	25·8	57	57	3·7 to 4·9	5d 8h 47m 39.974s
W Cephei	22	32·9	57	58	7 to 8	7 days
R Pegasi	23	02·1	10	03	6·9 to 13	380·0 days
S Pegasi	23	16·0	8	25	7·3 to 13	317·5 days
U Cephei	0	54	81	23	7·1 to 9·2	2d 11h 49m 38·25s

Radiant Points of Meteor Showers

Date	R. A.		Dec.	
	h	m	°	
September 3	23	36	38	Near 14 Andromedæ
September 7	4	08	37	Near ε Persei
September 17	5	36	48	The Aurigids; near β Aurigæ
September 21	2	04	19	Near 15 Arietis
September 22	0	28	10	Near δ Piscium
September 25	6	36	43	15° East of β Aurigæ
Sept. 25-Oct. 25	7	00	27	The Geminids; near τ Geminorum
October 2	15	00	52	Near κ Bootis
October 8	2	48	55	Near η Persei
October 11	0	52	7	Near ε Piscium
October 14	9	00	68	Near σ Ursæ Majoris
October 18	6	02	15	Orionids; near ζ Orionis
October 20	7	04	12	Near β Canis Minoris

Meridian Passage of the Principal Planets During September and October

LOCAL MEAN TIME

	For September 15		For October 15
Mercury	1.29 P.M.	Virgo	11.21 A.M.
Venus	2.09 P.M.	Virgo, Libra, Scorpio, Ophiuchus	2.32 P.M.
Mars	12.46 A.M.	Pisces	10.14 P.M.
Jupiter	12.05 P.M.	Leo, Virgo	10.30 A.M.
Saturn	1.51 A.M.	Pisces	11.40 P.M.
Uranus	7.37 A.M.	Sagittarius	5.40 P.M.
Neptune	7.46 A.M.	Gemini	5.50 A.M.

Clusters and Nebulæ

*M*71. Cluster. R. A. $19^h\ 49{\cdot}7^m$; Dec. 18° 32′; a large dim cloud to low powers, but on increase of power yields a mass of minute stars (11th to 16th magnitudes); an interesting specimen of the process of nebular resolution.

*M*27. The Dumb-Bell Nebula. R. A. $19^h\ 55{\cdot}7^m$; Dec. 22° 29′. This is one of the most beautiful of telescopic objects; in a rich field are two oval hazy masses in contact, of which the preceding is perhaps slightly brighter. With a large telescope the dark notches separating the two parts are filled in with faint nebulosity, converting the figure into an ellipse. There appear in it several minute stars, but these are probably not physically connected with it; its spectrum is purely gaseous.

H V.15. R. A. $20^h\ 41{\cdot}9^m$; Dec. 30° 23′; a nebulous ray extending north and south from 52 Cygni, a star of the fourth magnitude, with a companion of the ninth.

H V.14. R. A. $20^h\ 52{\cdot}6^m$; Dec. 31° 21′; a large nebulosity in a curve; photography shows that this and the preceding are probably parts of one large nebula.

H IV.1. Planetary Nebula. R. A. $20^h\ 59{\cdot}2^m$; Dec −11° 44′. Elliptic in form; longer diameter about 25″; very bright for an object of this nature; of a pale blue color, and bearing magnifying more like a planet than an ordinary nebula. It is one of the best specimens of planetary nebulæ.

At Greenwich Mean Noon

Date 1909	Day of Year	The Sun's Right Ascension	The Sun's Declination	Equation of Time Sub't from / Add to Mean Time	Sidereal Time
		h m s	° ′ ″	m s	h m s
Wed. 1	244	10 40 05·23	N. 8 25 51·0	0 03·37	10 40 01·86
Sat. 4	247	10 50 57·30	7 20 06·2	0 54·22	10 51 51·52
Tues. 7	250	11 01 47·26	6 13 16·2	1 53·93	11 03 41·19
Fri. 10	253	11 12 35·60	5 05 29·7	2 55·24	11 15 30·84
Mon. 13	256	11 23 22·77	3 56 55·3	3 57·74	11 27 20·51
Thur. 16	259	11 34 09.21	2 47 42·6	5 00·95	11 39 10·16
Sun. 19	262	11 44 55·38	1 38 01·0	6 04·44	11 50 59·82
Wed. 22	265	11 55 41·77	0 28 00·2	7 07·71	12 02 49·48
Sat. 25	268	12 06 28·86	S. 0 42 10·4	8 10·29	12 14 39·15
Tues. 28	271	12 17 17·25	1 52 21·4	9 11·55	12 26 28·80

75th Meridian Time. Hours numbering from Midnight

Planetary Phenomena

			Minima of Algol	Configuration of Jupiter's Satellites at 5h.
			h m	
Wed.	1	22h 39m ☌ ♂ ☾, ☌ 1° 4′ S.	11 4	
Thur.	2	23h 33m ☌ ♄ ☾, ♄ 1° 14′ N.		
Fri.	3			
Sat.	4		7 53	
Sun.	5			
☾ Mon.	6	14h 44·4m, Last Quarter.		
Tues.	7		4 42	
Wed.	8	1h ☿ in Aphelion.		
Thur.	9	6h 50m ☌ ♆ ☾, ♆ 3° 43′ S.		
Fri.	10	19h ☿ in ♌.	1 31	
Sat.	11			
Sun.	12		22 20	
Mon.	13			
☉ Tues.	14	10h 8·7m New Moon; 12h 51m ☌ ♃ ☽, ♃ 4° 10′ S.		
Wed.	15		19 09	
Thur.	16	4·5h ☾ in Apogee; 14h 23m ☌ ☿ ☾, ☿ 7° 10′ S.		
Fri.	17	5h ☿ Greatest Elongation E., 26° 34′; 16h 9m ☌ ♀ ☾,		
Sat.	18	9h ☌ ♃ ☉. [♀ 3° 55′ S.	15 58	
Sun.	19			
Mon.	20			
Tues.	21		12 47	
☽ Wed.	22	13h 31.5m First Quarter.		
Thur.	23	12h ☉ enters Libra, Autumn begins; 22h 3m ☌ ♅ ☾,		
Fri.	24	5h ☍ ☍ ☉. [♅ 2° 35′ N.	9 36	
Sat.	25			
Sun.	26	21h ♅ Stationary.		
Mon.	27	[☍ 0° 9′ N.	6 25	
Tues.	28	10h ☿ Greatest Hel. Lat. S.; 22h 2m ☌ ☍ ☾,		
☽ Wed.	29	8h 5·4m Full Moon; 12·0h ☾ in Perigee.		
Thur.	30	4h ☿ Stationary; 7h 1m ☌ ♄ ☾, ♄ 1° 8′ N.	3 14	

Key to Symbols.—☌ Conjunction; ☍ Opposition; □ Quadrature; ☊ Ascending Node; ☋ Descending Node; ☉ Sun; ☿ Mercury; ♀ Venus; ⊕ Earth; ♂ Mars; ♃ Jupiter; ♄ Saturn; ♅ Uranus; ♆ Neptune.

AT GREENWICH MEAN NOON

Date 1909	Day of Year	The Sun's Right Ascension	The Sun's Declination	Equation of Time Add to Mean Time	Sidereal Time
		h m s	° ′ ″	m s	h m s
Fri. 1	274	12 28 07·57	S. 3 02 24·1	10 10·89	12 38 18·46
Mon. 4	277	12 39 00·55	4 12 09·7	11 07·59	12 50 08·12
Thur. 7	280	12 49 56·69	5 21 29·5	12 01·08	13 01 57·78
Sun. 10	283	13 00 56·63	6 30 13·6	12 50·81	13 13 47·44
Wed. 13	286	13 12 00·80	7 38 11·9	13 36·30	13 25 37·10
Sat. 16	289	13 23 09·64	8 45 13·5	14 17·12	13 37 26·76
Tues. 19	292	13 34 23·56	9 51 07·7	14 52·86	13 49 16·42
Fri. 22	295	13 45 42·95	10 55 43·5	15 23·14	14 01 06·09
Mon. 25	298	13 57 08·20	11 58 50·0	15 47·55	14 12 55·75
Thur. 28	301	14 08 39 73	13 00 16·5	16 05·68	14 24 45·41
Sun. 31	304	14 20 18·03	13 59 53·3	16 17·05	14 36 35·08

75th Meridian Time, Hours numbering from Midnight

Planetary Phenomena

			Minima of Algol	Configuration of Jupiter's Satellites at 5h.
			h m	
	Fri. 1			Invisible on account of proximity to Sun.
	Sat. 2			
	Sun. 3		0 03	
	Mon. 4			
	Tues. 5		20 52	
☾	Wed. 6	1h 44·2m Last Quarter; 13h 33m ☌ ♅ ☾, ♅ 4° 0′ S.		
	Thur. 7			
	Fri. 8		17 41	
	Sat. 9			
	Sun. 10	23h □ ♁ ☉.		
	Mon. 11		14 30	
	Tues. 12	6h 58m ☌ ♃ ☾ 4° 3′ S.; 10h ☌ ☿ ☉ Inferior.		
	Wed. 13	2h □ ♅ ☉; 6·6h ☾ in Apogee; 13h ☍ ♄ ☉; 16h 19m		
☽	Thur. 14	3h 13·4m New Moon. [☌ ☿ ☾ ♀ 5° 50′ S.	11 19	
	Fri. 15	6h ♀ in Aphelion.		
	Sat. 16			
	Sun. 17	11h ☿ in ♌; 23h 40m ☌ ♀ ☾, ♀ 2° 17′ S.	8 08	31○4●
	Mon. 18			4○312
	Tues. 19			412○3
	Wed. 20	20h ☿ Stationary.	4 57	42○13
	Thur. 21	5h 34m ☌ ♁ ☾, ♁ 2° 48′ N.		4○32●
☽	Fri. 22	1h ☿ in Perihelion; 2h 3·6 m First Quarter; 22h ♅		431○2
	Sat. 23	[Stationary.	1 45	432○1
	Sun. 24			431○●
	Mon. 25		22 34	4○312
	Tues. 26	1h 6m ☌ ♂ ☾, ♂ 2° 38′ N.; 12h ♂ Stationary.		12○43
	Wed. 27	14h 48m ☌ ♄ ☾ 1° 17′ N.; 23·7h ☾ in Perigee.		2○143
☽	Thur. 28	2h ☿ Greatest Elongation W., 18° 33′; 17h 7·0m Full	19 23	1○324
	Fri. 29	[Moon.		♃3○24
	Sat. 30			32○14
	Sun. 31		16 12	312○4

For Jupiter's Satellites, the circle ○ represents the disc of the planet; ♃ signifies that the satellite is on the disc; ● signifies that the satellite is behind the disc or in the shadow. Configurations are for an inverting telescope.

7027. Planetary Nebula. R. A. $21^h\ 3{\cdot}6^m$; Dec. 41° 52′. About 4″ in diameter; brightness equivalent to 8·5 magnitude.

*M*15. Cluster. R. A. $21^h\ 25{\cdot}5^m$; Dec. 11° 45′. Fine specimen of a completely insulated cluster; resolvable into 15th magnitude stars; brightest in centre; a dark patch near middle, with two faint dark lanes.

*M*2. Resolvable Nebula. R. A. $21^h\ 28{\cdot}8^m$; Dec. −1° 14′. Round; diameter about 5′, showing in a small telescope a granular aspect, the precursor of resolution. Condensed towards the centre; probably consisting of thousands of 15th magnitude stars.

*M*30. Cluster. R. A. $21^h\ 35{\cdot}2^m$; Dec. −23° 36′. Moderately bright; beautifully contrasted with an 8th magnitude star beside it; comet-like with low powers; with higher power resolvable into 12th magnitude stars. It lies closely preceding, and a little north from, 41 Capricornus, a 5th magnitude star.

Occultations of Stars by the Moon*

Date	Star	Mag.	Time				Position Angle	
			Immersion		Emersion		Immersion	Emersion
			h	m	h	m	°	°
Sept. 21	θ *Ophiuchi*	3·3	9	1·6			129	
" 25	33 *Capricorni*	5·3	12	49·8			96	
" 28	33 *Piscium*	4·7	6	15·9	7	12·4	66	245
" 28	24 β *Ceti*	6·0	9	3·0	9	52·0	102	194
" 20	26 *Ceti*	6·0	8	2·2	9	0·7	45	256
" 29	33 *Ceti*	6·1	11	54·4	13	3·9	43	246
" 29	*f Piscium*	5·1	16	14·6	17	13·6	48	254
Oct. 20	σ *Sagittarii*	2·3	7	13·7	7	54·2	23	321
" 24	69 *Aquarii*	5·6	8	25·0	9	37·0	51	245
" 24	7 *Aquarii*	4·4	9	58·7	10	48·2	13	282
" 28	5 *Arietis*	5·5	6	1·0	6	51·5	43	262
" 28	31 *Arietis*	5·7	12	35·4	13	23·9	13	287
" 31	121 *Tauri*	5·1	10	38·8	11	35·3	102	226
" 31	132 *Tauri*	5·0	17	50·6	18	50·1	119	237

*Prepared by R. M. Motherwell for use at the Dominion Observatory.